The Welfare Implications of Insurance Bad Faith Liability: Evidence from the *Royal Globe* Doctrine

Brian Richman
Department of Policy Analysis & Management
Cornell University
Ithaca, NY 14853
bar57@cornell.edu

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Research Mentor: Sharon Tennyson
Committee Member: Rick Geddes

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Abstract

This paper analyzes the effects of the *Royal Globe* doctrine (increased insurance bad faith liability) on automobile bodily injury insurance claims outcomes. The study provides new evidence over a wide range of claim outcomes to paint a fuller picture of the effects of increased bad faith liability. The use of a "natural experiment" created by the "random" nature of the *Royal Globe* decision and the utilization of the powerful synthetic control method allows the study to make causal estimates on observed claims settlement practices, an advantage over previous work. The results suggest that despite some previous findings, insurers are not *too* deterred that they start paying more fraudulent claims and doing less investigations, and that the true beneficiaries of bad faith liability may be those small claims that absent the additional punitive damages would have no incentive to sue for claims underpayment. Overall, the results indicate that bad faith liability may be efficiency enhancing.

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1 Introduction

> An insurance policy is not an ordinary contract. It is a complex instrument, unilaterally prepared and seldom understood by the insured. The parties are not similarly situated... In short, the insurance company may not ignore its insured and then seek refuge in the fine print of its policy.
>
> - *Allstate Ins. Co. v. Pietrosh* (Nev. 1969)[1]

Courts and legislatures have long recognized the "special relationship between the insurer and the insured"[2]and have thus held insurers to a standard of "good faith and fair dealing"[3] with policyholders.[4] Until the 1970s, however, policyholders were limited under English common law[5] from recovering beyond the limits of their insurance policies,[6] even if the insurer intentionally breached the contract.[7] Eventually jurisdictions began to expand the ability of policyholders to recover for unfair treatment, but substantially differed in opinion on how to best protect consumers.[8]

Depending on the situation, jurisdiction, and time, courts relied on different strategies, such as, tort actions based solely on bad faith, contract actions with a

[1] 85 Nev. 310, 316 (1969).

[2] *Jonathan Neil & Assoc., Inc. v. Jones*, 94 P.3d 1055, 1068 (Cal. 2004).

[3] See *Thompson v. Shelter Mut. Ins.*, 875 F.2d 1460 (10th Cir. 1989); *Weese v. Nationwide Ins. Co.*, 879 F.2d 115 (4th Cir. 1989).

[4] See *Rodgers v. Pennsylvania Life Ins. Co.*, 539 F. Supp. 879, 883 (S.D. Iowa 1982) ("The superior bargaining power of an insurance company over its insured has long been recognized in Iowa."); *Noble v. National Arm. Life Ins. Co.*, 624 P.2d 866, 867 (Ariz. 1981) ("The special nature of an insurance contract has been recognized by courts and legislatures for many years."); *Grand Sheet Metal Prods. Co. v. Protection Mut. Ins. Co.*, 375 A.2d 428, 430 (Conn. Super. Ct. 1977) ("[T]he unequal bargaining power of the parties, the special nature of the insurance business, and the disastrous economic effects that a bad faith refusal to pay may cause the insured are paramount considerations.").

[5] See *Hadley v. Baxendale*, 156 Eng. Rep. 145 (Ex. 1854).

[6] See *Mannheimer Bros. v. Kansas Cas. Co.*, 149 Minn. 482 (1921).

[7] *Gruenberg v. Aetna Insurance Company*, 9 Cal. 3d 566 (1973), was the first case to allow plaintiffs to apply tort liability to a first-party insurers bad faith.

[8] See Vance (1951); Stempel (2006); Tennyson and Warfel (2009) for a discussion of the development of insurance bad faith liability.

broad definition of damages,[9] private causes of action based on statute,[10] and the general "competition among insurers,"[11] to protect consumers from unfair insurance industry practices. Even among the jurisdictions that allow tort actions based solely on bad faith, there are various standards for determining bad faith, such as, "negligence,"[12] "intentional tort,"[13] or a "quasi-criminal"[14] standard (Tennyson and Warfel, 2009). Despite the recent leveling off of bad faith liability when compared to the "exciting days of the late 1970s and 1980s" (Abraham, 1994), the policy implications for the degree of bad faith liability that insurers face are still relevant (Jerry, 1994; Tennyson and Asmat, 2010).

There are many recent examples of jurisdictions changing or establishing insurance bad faith regimes. For example, in 2007 Washington expanded the definition of bad faith and increased the potential damages that policyholders could recover[15] and in 2008 Minnesota created a new private cause of action for first-party insurance bad faith.[16] Further, the intense discussion surrounding the contentious passage of the Patient Protection and Affordable Care Act ("the ACA" or "Obamacare")[17] shows that the issue of consumer protection from potential unfair insurance industry practices - an industry "legendary" for its "miserly proclivities" regarding settlement claims (Crocker and Tennyson, 2002, 470) - is of great importance and debate.

[9]See *Beck v. Farmers Insurance Exchange*, 701 P.2d 795 (Utah 1985).

[10]See Conn. Gen. Stat. 38a-816, 42-110q (2009); *Maher v. Contl Cas. Co.*, 76 F.3d 535 (4th Cir. 1999) (applying West Virginia law to allow unlimited punitive damages against an insurer who engaged in one or more statutorily prohibited practices).

[11]See *Jonathan Neil & Assoc., Inc. v. Jones*, 16 Cal. Rptr. 3d 849, 866 (2004) (the California Supreme Court observed that, "generally speaking, the insurers ability to charge excessive premiums will be disciplined by competition among insurers.").

[12]See *Comunale v. Traders & Gen. Ins. Co.*, 328 P.2d 198 (Cal. 1958)

[13]See *Anderson v. Contl Ins. Co.*, 271 N.W.2d 368 (Wis. 1978)

[14]See *Aetna Casualty and Surety v. Broadway Arms*, 664 S.W.2d 463 (Ark. 1984).

[15]Wash. Rev. Code 48.30.010 (2007).

[16]2008 Minn. Laws 604.18.

[17]P.L. 111-148 (2010).

Despite the intense debate surrounding insurance bad faith liability, the net effect on social welfare from increased bad faith liability is unclear. An increase in tort liability greatly increases the potential damages faced by an insurer for delaying or underpaying a claim (Abraham, 1986, 1994; Sykes, 1996; Crocker and Tennyson, 2002; Browne et al., 2004; Tennyson and Warfel, 2009; Tennyson and Asmat, 2010). Unlike contract law, tort law allows for the recovery for all harm or injuries sustained (including legal expenses, economic loss, and mental distress) and has the potential for punitive damages. Contract law, however, does not allow for punitive damages and is restricted to the general or consequential damages resulting from, or reasonably foreseeable from, the original breach of contract (Garner, 1999; Tennyson and Warfel, 2008, 2009).[18] Economic theory therefore predicts that an increase in the expected judgments against insurers resulting from an increase in exposure to bad faith liability will reduce the incentive of insurers to deny, delay, or underpay claims (Abraham, 1986, 1994; Sykes, 1996; Crocker and Tennyson, 2002; Tennyson and Warfel, 2009; Tennyson and Asmat, 2010).[19]

The incentive created by increased bad faith liability to not deny, delay, or underpay claims *may or may not* be efficiency enhancing.[20] The incentive is efficiency enhancing if it discourages insurers from underpaying *legitimate* claims. The same incentive to not underpay claims, may, however, encourage insurers to pay "*reasonably disputable claims*" (emphasis added) (Abraham, 1986; Crocker and Tennyson, 2002; Tennyson and Warfel, 2009; Tennyson and Asmat, 2010). Paying "reasonably disputable claims" is efficiency diminishing as it may increase the

[18]Posner (2011, 170-174) explains why punitive damages are "rarely awarded in contract cases."

[19]See *Bibeault v. Hanover Ins. Co.*, 417 A.2d 313, 318 (R.I. 1980) ("Recovery under a contract theory aloneeffectively guards an insurers pocketbook against any threat of punitive damages. In this atmosphere, insurers, backed by sufficient financial resources, are encouraged to delay payment of claims to their insureds with an eye toward settling for a lesser amount than that due under the policy.").

For classic articles on deterrence, see Becker (1968) and Stigler (1970).

[20]Landes and Posner (1987, 1) describe the positive economic theory of tort law as if "judges who created the law through decisions operating as precedents in the later cases were trying to promote efficient resource allocation."

number of fraudulent claims that insurers receive and pay (Tennyson and Warfel, 2009). To date, the empirical literature is not conclusive as to the net welfare implications of increased bad faith liability.

This paper attempts to shed some light on the issue by taking advantage of a unique "natural experiment" created by the California Supreme Court in *Royal Globe Insurance Company v. Superior Court*.[21] The "unprecedented" *Royal Globe* doctrine allowed, for the first time, third-party bad faith suits (Casey, 1983). That is, a third party to an insurance contract (neither the insurer nor the insured) could bring a tort claim against an insurer for acting in bad faith to settle the third party's claim against the insured. This decision greatly expanded the potential bad faith liability faced by insurers in only California (Gainer, 1989; Abraham, 1994; Hawken et al., 2001).[22] By analyzing individual automobile insurance claims that were settled before and after the *Royal Globe* decision and using a synthetic control procedure (see Abadie and Gardeazabal, 2003; Abadie et al., 2010), I calculate difference-in-differences estimates for measures of claims settlements outcomes between California and a synthetic "California" made up of a weighted average of potential control states. The strength of this method allows me to examine the causal effects of increased bad faith liability.

The remainder of the paper proceeds as follows. The next section, Section 2, examines the evolution of bad faith liability in the courts. Section 3 then discusses the predictions of the economics literature on the effects of expanded bad faith liability. I then turn, in Section 4, to a discussion of the previous empirical literature and a description of the data and methods used in this study. Finally, Section 5 presents the empirical results and Section 6 offers the conclusions.

[21] 23 Cal. 3d 880 (1979).

[22] The California Supreme Court overturned the *Royal Globe* decision in *Parvaneh Moradi-Shalal v. Firemans Fund Insurance Companies*, 785 P.2d 250 (1988).

2 Legal Analysis

2.1 The Implied Covenant of Good Faith and Fair Dealing

The concept of an implied duty of good faith in contracts existed in the law for at least the previous two thousand years (Jerry, 1994). Under Roman law, the *ex fide bona* clause authorized judges to find an obligation for both parties to a consensual contract to act in good faith (Sohm et al., 1907; Jerry, 1994). Further, in the early part of the seventeenth century, the writ of assumpsit under English common law expanded to allow local courts to emphasize the "duties of good faith and conscience" in contractual relations (Powell, 1956; Jerry, 1994). Eventually in the eighteenth century under the guidance of Lord Mansfield, "contract law briefly flirted" with the idea that a consideration was a feeling that one was morally obligated (Jerry, 1994).[23] As Lord Mansfield stated in *Carter v. Boehm*:[24]

> The governing principle is applicable to all contracts and dealings. Good faith forbids either party by concealing what he privately knows, to draw the other into a bargain, from his ignorance of that fact, and his believing the contrary.

The notion of an implied duty of good faith entered American common law at around the same time it entered English common law in the late eighteenth century. Even though American courts relied on the doctrine of *caveat emptor* ("let the buyer beware") more than English courts, the duty of good faith always remained in the United States (Jerry, 1994).[25] Overtime, thousands of courts stated that there is an implied covenant of good faith and fair dealing in every contract.[26] For exam-

[23] In the eighteenth century, both David Hume and Adam Smith viewed an implicit duty to good faith as existing in every contract (Atiyah, 1979).

[24] 3 Burr 1905 (1766).

[25] See *Industrial & General Trust v. Tod*, 73 N.E. 7 (N.Y. 1905) ("No one can be made by contract the final judge of his own acts, for the law writes good faith into such agreements").

[26] See *Western oil & Fuel Co. v. Kemp*, 245 F.2d 633 (8th Cir. 1957); *L.L. Hall Constr. Co. v. United States*, 379 F.2d 559 (1966).

ple, in *Kirke La Shelle Co. v. Paul Armstrong Co.*[27] the New York Court of Appeals ruled that:

> [I]n every contract there is an implied covenant that neither party shall do anything which will have the effect of destroying or injuring the right of the other party to receive the fruits of the contract, which means that in every contract there exists an implied covenant of good faith and fair dealing.

This statement is "hornbook law of contract" for both insurance and non-insurance contracts (Stempel, 2006; American Law Institute, 1981).[28]

As Posner (2009) explains, the duty of good faith does not impose a "moral duty" to complete a contract. There are many situations where the completion of a contract is impossible at a reasonable cost[29] or the breach of contract is efficient[30] (see Posner, 2011, 149-158). Rather, the duty of good faith is "just a duty to avoid exploiting the temporary monopoly position that a contracting party will sometimes obtain during the course of performance" (Posner, 2009, 1358). When parties to a contract act sequentially (as opposed to simultaneously), one party may put himself under the power of the other party. Consider, for example, a situation

[27] 188 N.E. 163 (N.Y. 1933).

[28] The Restatement (Second) Contracts 205 (American Law Institute, 1981) states that every "contract imposes upon each party a duty of good faith and fair dealing in its performance and its enforcement."

[29] see e.g., *Ridgely v. Conewago Iron Co.*, 53 Fed. 988 (E.D. Pa. 1893) (holding that a mining lease requiring the lessee to mine four thousand tons of ore annually for a fixed sum per ton, or, failing to mine the minimum quantity, to pay the fixed sum for the minimum quantity, did not obligate the lessee to pay for the additional quantity after the ore on the premises was exhausted); *Transatlantic Financing Corporation v. United States*, 363 F.2d 312, 315 (D.D.C. 1966) ("The doctrine of impossibility of performance has gradually been freed from the earlier fictional and unrealistic strictures of such tests as the implied term and the parties contemplation. It is now recognized that a thing is impossible in legal contemplation when it is not practicable; and a thing is impracticable when it can only be done at an excessive and unreasonable cost.").

[30] see e.g., *Patton v. Mid-Continent Systems, Inc.*, 841 F.2d 742, 750 (7th Cir. 1988) ("Suppose that by franchising Truck-O-Mat in the plaintiffs' territory [which breaches the contract with plaintiff], Mid-Continent increased its own profits by $150,000 and inflicted damages of $75,000 on the plaintiffs. That would be an efficient breach. But if Mid-Continent had known that it would have to pay in addition to compensatory damages $100,000 in punitive damages, the breach would not have been worthwhile to it and efficiency would have suffered because the difference between Mid-Continent's profits of $150,000 and the plaintiffs' losses of $75,000 would (certainly after the plaintiffs were compensated) represent a net social gain.").

where A pays his insurance premiums to insurer B, who agrees to compensate A for any fire damage to his house in the future. Then, if A's house burns down and his only source of finances is the payment owed to him by B, A may agree to a lower payment due to B's monopoly position in the negotiations (see Posner, 2009, 1358-1359).[31]

2.2 Development of Bad Faith Liability

In almost all jurisdictions, for all non-insurance contracts, and for insurance contracts before the 1950s, courts refused to recognize a separate cause of action for bad faith breach of contract (Stempel, 2006).[32] Pursuant to English common law, as stated in *Hadley v. Baxendale*,[33] insurers were only liable for damages up to the policy limits. Despite the special relationship between the insurer and the insured, and the insurers subsequent "duty to exercise good faith and diligence"[34] when dealing with the insured, courts refused to extend liability beyond the policy lim-

[31]For a similar non-insurance case, see *Alaska Packers' Ass'n v. Domenico et al.*, 177 F. 99, 102 (9th Cir. 1902) ("A party who refuses to perform, and thereby coerces a promise from the other party to the contract to pay him an increased compensation for doing that which he is legally bound to do, takes an unjustifiable advantage of the necessities of the other party. ...There can be no consideration for the promise of the other party, and there is no warrant for inferring that the parties have voluntarily rescinded or modified their contract. The promise cannot be legally enforced, although the other party has completed his contract in reliance upon it.")

[32]See *Sherrin v. Northwestern Natl Life Ins. Co.*, 2 F.3d 373, 381 (11th Cir. 1993) (applying Alabama law) ("[P]unitive damages are ordinarily not recoverable for breach of contract"); *Jacobs v. Farmland Mut. Ins. Co.*, 377 N.W.2d 441, 444 (Minn. 1985) ("[I]f plaintiffs cause of action is for breach of contract, the damages recovered for the breach will not support an award of punitive damage unless the breach also constitutes or is accompanied by an independent tort"); Holmes (1897, 462) ("The duty to keep a contract at common law means a prediction that you must pay damages if you do not keep it - and nothing else").

[33]156 Eng. Rep. 145 (Exch. Div.) (1854) ("It follows, therefore, that the loss ...here cannot reasonably be considered such a consequence of the breach of contract as could have been fairly and reasonably contemplated by both the parties when they made this contract. For such loss would neither have flowed naturally from the breach of this contract in the great multitude of such cases occurring under ordinary circumstances, nor were the special circumstances, which, perhaps, would have made it a reasonable and natural consequence of such breach of contract, communicated to or known by the defendants. ...[T]herefore, ...[the Court] ought not to take the loss ...into consideration at all in estimating the damages.").

[34]*Brassil v. Maryland Cas. Co.*, 210 N.Y. 235 (1914).

its even for an intentional breach of contract (Vance, 1951, 1004-1005).[35] In order to win greater damages plaintiffs had to file separate claims for an additional tort, such as intentional infliction of emotional distress or fraud (Sykes, 1996).

Eventually in 1958, the California Supreme Court first addressed the issue of bad faith liability for third party insurance in *Comunale v. Traders & General Insurance Co*.[36] Third party insurance or liability insurance provides coverage for losses that the insured party caused to another party (Garner, 1999, 806).[37] The Court in *Comunale* held that the "implied covenant of good faith and fair dealing requires the insurer to settle [a claim against the insured] in an appropriate case although the express terms of the of the policy do not impose such a duty."[38] Because the insurer "rejected a reasonable offer of settlement" and "wrongfully refused to defend" the insured, the Court held the insurer liable for amounts in excess of the policy limits.[39] The rationale for increasing the potential liability faced by third party insurers for bad faith claims settlements is best explained by the Second Circuit in *Pinto v. Allstate Ins. Co*.:[40]

> An insurer has an economic incentive not to settle, hoping that a jury will bring in a verdict for less than the policy limits. But when such hope goes awry... the insured is the loser, being personally responsible for the excess. These conflicting interests between the insurer and the insured cause them to rub against each other like unmoored rowboats on a placid pond.

Nine years after *Comunale*, in 1967, the California Supreme Court allowed for a separate cause of action under tort liability against an insurer that exercised bad faith in settling a claim against one of its insured (Price, 1980). While relying on

[35] *Mannheimer Bros v. Kansas Cas. Co.*, 149 Minn. 482 (1921).

[36] 50 Cal. 2d 654 (1958).

[37] See the Symposium on "The Law of Bad Faith in Contract and Insurance" in the *Texas Law Review* (1994), 72(6), 1203-1702, for a full discussion of the development and implications of insurance bad faith liability.

[38] 50 Cal.2d 654, 659.

[39] *Id*. at 660.

[40] 221 F.3d 394, 396 (2d Cir. 2000) (applying New York law).

the ruling in *Comunale* that the "implied obligation of good faith and fair dealing requires the insurer to settle in an appropriate case,"[41] the Court in *Crisci v. Security Insurance Co.*,[42] imposed liability on the insurer "not for bad faith breach of the contract but for failure to meet the duty to accept reasonable settlements, a duty included within the implied covenant of good faith and fair dealing." According to the Court, the breach of contract in the *Crisci* case "also constitutes a tort."[43]

Today, 48 state courts have found an express cause of action for third-party bad faith (Syverud 1990, 1120; Stempel 2006, §9, 100).[44] A minority of those 48 states[45] utilize a negligence based test to determine if an insurer breached its duty to settle.[46] The Court in *Crisci*, which adopted a negligence based test, explained the criteria as, "liability may exist when the insurer unwarrantedly refuses an offered settlement where the most reasonable manner of disposing of the claim is by accepting the settlement."[47] The majority of courts, however, do not use the negligence standard. Instead, these courts rely on a breach of the covenant of good faith and fair dealing. Specifically, these states either require an affirmative bad faith[48]

[41] 50 Cal. 2d 654, 659 (1958).

[42] 66 Cal. 2d 425, 430 (1967).

[43] *Id.* at 434.

[44] The District of Columbia, Nevada, and West Virginia have never expressly accepted or rejected a third-party bad faith tort cause of action (see Stempel 2006, §9, 94 and Syverud 1990, 1120).

[45] See Stempel (2006, §9, 100); *Robertson v. Hartford Acc. & Indem. Co.*, 333 F. Supp. 739 (D. Or. 1970); *Aetna Cas. & Sur. Co. v. Kornbluth*, 28 Colo. App. 194 (1970); *Stetler v. Fosha*, 809 F. Supp. 1409 (D. Kan. 1992); *United States Fire Ins. Co. v. Morrison Ass. Co.*, 600 So. 2d 1147 (Fla. Dist. Ct. App. 1992).

[46] Windt (1988, 259) argues that although a negligence standard is the de jure minority rule, it is the de facto majority rule because many cases have "in effect, held insurers liable even though they acted in good faith."

[47] 66 Cal. 2d 425, 430 (1967).

[48] See *State Farm Mut. Auto Ins. Co. v. Floyd*, 366 SE 2d 93, 97 (Va. Sup. Ct. 1988) ("[A]n insured . . . is required to show that the insurer acted in furtherance of its own interest, with intentional disregard of the financial interest of the insured."); *Commercial Union Ins. Co. v. Liberty Mut. Ins. Co.*, 137 Mich. App. 381, 392 (1984) (quoting *Wakefield v. Globe Indemnity Co.*, 246 Mich. 645, 652 (1929)) ("[T]he insurer does not act in bad faith if it refuses settlement in the honest belief that it had a fair chance of victory, or keeping the verdict within the policy limit, or that the compromise amount is excessive, or if it has legal defenses. On the other hand, arbitrary refusal to settle for a reasonable amount, where it is apparent that suit would result in a judgment in excess of the policy limit, indifference to the effect of refusal on the insured, failure to fairly consider a compromise and facts presented and pass honest judgment thereon or refusal to settle upon grounds which depart from the contract and the purpose of the grant of power, would tend to show bad faith."); *Diblasi*

or a failure to adequately consider the policyholder's interests (Stempel, 2006, §9, 100-101).[49]

Most of the early bad faith claims were claims against third party insurers.[50] It was not until 1973 that a state Supreme Court extended the tort of bad faith to first party insurance.[51] First-party or indemnity insurance is a policy that "applies to oneself or ones own property," such as health, disability, and fire insurance (Garner, 1999, 804). Again, leading the way, the California Supreme Court in *Gruenberg v. Aetna Insurance Co*.[52] found that the insurers duty to settle in first-party and third-party insurance claims were "merely two different aspects of the same duty." The Court reasoned that if an insurer:

> ...fails to deal fairly and in good faith with its insured by refusing, without proper cause, to compensate its insured for a loss covered by the policy, such conduct may give rise to a cause of action in tort for breach of an implied covenant of good faith and fair dealing.[53]

This tort action based solely on bad faith has the potential for the highest dam-

v. Aetna Life & Cas. Ins. Co., 147 A.D.2d 93, 99 (1989) ("[T]here is a cause of action only if the decision not to settle within the policy limits was made in bad faith, meaning in gross disregard of its insured's interests").

[49]See *Hayes Bros., Inc. v. Economy Fire & Cas. Co.*, 634 F.2d 1119, 1123 (1980) (An insurer must "give equal consideration to the interest of its insured, as well as itself, in evaluating a settlement demand."); *Baton v. Transamerica Ins. Co.*, 584 F. 2d 907, 912 (9th Cir. 1978) (applying Delaware law) (quoting *Eastham v. Oregon Auto Ins. Co.*, 273 Or. 600, 607 (1975)) ("Good faith requires the insurer, in handling negotiations for settlement, to treat the conflicting interests of itself and the insured with impartiality, giving equal consideration to both interests.").

[50]See *Gray v. Zurich Ins. Co.*, 65 Cal. 2d 263, 274 (1966) ("[W]e hold that in the present case the policy provides for an obligation to defend and that such obligation is independent of the indemnification coverage.").

[51]*Gruenberg v. Aetna Ins. Co.*, 9 Cal. 3d 566 (1973); The first court to uphold tort liability for bad faith in first-party insurance was the Court of Appeals of California in *Fletcher v. Western Natl Life Ins. Co.*, 10 Cal. App. 3d 376, 401 (4th Dist. 1970) ("We hold that defendants threatened and actual bad faith refusals to make payments under the policy, maliciously employed by defendants in concert with false and threatening communications directed to plaintiff for the purpose of causing him to surrender his policy or disadvantageously settle a nonexistent dispute is essentially tortuous in nature and is conduct that may independent of the tort of intentional infliction of emotional distress constitute[] a tortuous interference with a protected property interest of its insured for which damages may be recovered to compensate for all detriment proximately resulting therefrom, including economic loss as well as emotional distress resulting from the conduct or from the economic losses caused by the conduct, and, in a proper case, punitive damages.").

[52]9 Cal. 3d 566, 573 (1973).

[53]*Id.* at 574.

ages against the insurers as the injured party can recover for all damages (e.g., economic losses, mental distress, legal fees, etc.), "regardless of whether these damages could have been anticipated" (Tennyson and Warfel, 2009, 205). Of the states adopting the tort action based solely on bad faith, a minority use the "negligence" (an insurer must consider the insured's interest and its own when considering a settlement offer)[54] or a "quasi-criminal" standard ("evidence of bad faith must be sufficient to show affirmative misconduct of a nature which is malicious, dishonest, or oppressive").[55] A majority of the states use an "intentional tort" standard, which, as expressed for the first time by the Supreme Court of Wisconsin in *Anderson v. Continental Insurance Co.* (1978),[56] defines the tort of bad faith as "a separate intentional wrong, which results from a breach of duty imposed as a consequence of the relationship established by contract"[57] (see Tennyson and Warfel, 2009, 207-213).

In addition to the tort based standard for first-party insurance bad faith liability, other states have relied on contract actions with a broad definition of damages or statutes. The contract action with the broad definition of damages was first adopted in *Beck v. Farmers Insurance Exchange*.[58] The Court reasoned that the policy limits do not restrict the amount that the insurer can be held liable for a breach of contract. Unlike the tort standard, however, an insurer can not face punitive damages unless a separate tort is also alleged (e.g., fraud) (Tennyson and Warfel, 2009, 214). Finally, some states have adopted a private cause of action based on statute for first-party insurance bad faith liability. Minnesota in 2008, for example, passed legislation implementing the intentional tort standard, establishing certain

[54] According to Tennyson and Warfel (2009, 207, n24), at least eleven states use the negligence standard. See, for example, *State Farm Fire Cas. Co. v. Nicholson*, 777 P.2d 1152 (Alaska 1989).

[55] *Aetna Casualty and Surety v. Broadway Arms*, 664 S.W.2d 463, 467 (Ark. 1984). Arkansas is the only state to use a "quasi-criminal" standard.

[56] 271 N.W.2d 368 (Wis. 1978).

[57] *Id.* at 374.

[58] 701 P.2d 795 (Utah 1985).

prerequisites for damages, banning punitive damages, and specifying costs and attorney's fees that people could recover under a newly created private cause of action for first-party insurance bad faith (Tennyson and Warfel, 2009, 215-217).[59]

2.3 The *Royal Globe* Doctrine

In 1979, the California Supreme Court in the "landmark" (Casey 1983, 917; Aitken and Abeltin 1987, 55) case *Royal Globe Insurance Co. v. Superior Court*[60] recognized, "for the first time" (Aitken and Abeltin, 1987, 55), the right for third parties to an insurance contact (not the insurer nor the insured) to bring tort actions against an insurer for that insurer's bad faith handling of a claims settlement. A simple example is useful to show the novelty of the *Royal Globe* doctrine: *A*, a careful driver, is legally driving his car through an intersection at the correct time and speed. *B*, an unsafe driver, then illegally drives through a red light and hits *A*. The evidence is clear that *B* is liable for *A*'s injuries and *B* has liability insurance up to $10,000 through insurer *C*. *A* makes a settlement offer of $5,000 (significantly below *B*'s policy limit), which is then rejected by *B*'s insurer, *C*, who at this point takes over the defense. This is because the "standard liability policy provides that the insurer 'shall defend any suit' alleging liability that would be covered by the policy if the suit proved successful, even if the suit is 'groundless, false, or fraudulent"' (Abraham, 1986, 195).[61] At trial, however, *A* wins a judgment against *B* for $50,000 and insurer *C* pays $10,000 to *A* (*B*'s policy limit). *B* is then liable for the remaining $40,000 of the judgment, which *A* has not received yet.

The above hypothetical is a typical example of an insurers bad faith in settling

[59] Minn. Stat. Ann. 604.18.

[60] 23 Cal. 3d 880 (1979).

[61] The liability insurer is required to defend the insured against any action brought, within the coverage of the policy (Vance, 1951, 1004). See e.g., *Comunale v. Traders & General Insurance Co.*, 50 Cal. 2d 654, 657 (1958) ("[The indemnity insurer] was obligated to defend any personal injury suit covered by the policy, but it was given the right to make such settlement as it might deem expedient.").

third party claims (see Tancredi, 1980). Before the *Royal Globe* doctrine, *B* would have to sue insurer *C* for bad faith to recover the additional $40,000 to eventually pay *A*. *A*, after the initial judgment, would have no recourse except to wait for *B* to acquire the money necessary to pay the remaining verdict.[62] After *Royal Globe*, however, in California, *A* would be able to file a separate bad faith claim against insurer *C*, after the liability of the insured is first determined, to recover the damages owed to him (and potential punitive damages).

The *Royal Globe* decision was completely unexpected. Only three years prior to *Royal Globe* the California Supreme Court unanimously ruled in *Murphy v. Allstate Ins. Co.*[63] that the duty of good faith and fair dealing did not extend to a third party. According to the Court, "[a] third party should not be permitted to enforce covenants made not for his benefit, but rather for others."[64] The Court in *Murphy*:

> ...firmly established that the duty of the insurer to settle a bona fide claim runs only to the insured and not to the injured claimant. The remedy of the injured party was clear: a suit against the insured. The insured, in turn, had standing to sue the carrier directly for a breach of the implied covenant of fair dealing and good faith if the insurer has wrongfully refused to settle and a judgment exceeding the policy limits was rendered (Meskin 1985, 378-379, quoting Price 1980, 1166-1167).

Based on the unanimous precedent set by the Court in *Murphy*, it was unexpected that the same Court in *Royal Globe* would come to a conclusion three years later that was seemingly "contrary to prior law."[65]

The four to three majority in *Royal Globe* got around the *Murphy* decision by finding that:

> In the present case, plaintiff does not seek to rely upon the violation of the insurers duty to its insured to settle plaintiffs claim. Rather, she

[62] See *Murphy v. Allstate Ins. Co*, 17 Cal. 3d 937 (1976).
[63] *Id.*
[64] *Id.* at 944.
[65] *Royal Globe Ins. Co. v. Superior Court*, 23 Cal.3d 880, 892 (1979) (RICHARDSON, J., concurring and dissenting).

> relies upon the insurer's duty owed to her as a claimant under [the Unfair Practices Act] subdivisions (h)(5) and (h)(4) of section 790.03, a duty created by those statutory provisions and owed directly to plaintiff as claimant.[66]

The subdivisions (h)(5) and (h)(4) of section 790.03 that the Court relied on in its decision to "create or authorize[] the direct action against the insurer by the injured party"[67] were added to Californias Unfair Practices Act (1959)[68] in 1972.[69] The decision in *Murphy*, however, was handed down in 1976. As Meskin (1985, 381-382) points out, the California Supreme Court "had the same opportunity in 1976 as it did in 1979 to recognize this duty based upon the statute. In essence, the *Royal Globe* court used an administrative statutory scheme to extend judicial law in a direction that it had expressly refused to go only three years earlier."

Further, according to Justice Richardson's concurrence and dissent, "neither" section 790.03 nor section 790.09 that the majority relied on in their decision, "creates or authorizes the direct action against the insurer by the injured party."[70] Section 790.03 describes "unfair claims settlement practices"[71] and section 790.09 preserves any already existing "administrative," "civil," and/or "criminal" liability.[72] Eventually, ten years later in *Moradi-Shalal v. Firemans Fund Ins. Companies*,[73] the California Supreme Court overruled its own decision in *Royal Globe* because that decision "incorrectly evaluated the legislative intent underlying the passage of section 790.03, subdivision (h)." No other state, except West Virginia in 1981[74]

[66] 23 Cal. 3d 880, 890 (1979).

[67] *Royal Globe Ins. Co. v. Superior Court*, 23 Cal.3d 880, 893 (1979) (RICHARDSON, J., concurring and dissenting).

[68] See Cal. Ins. Code 790 (LexisNexis 2009).

[69] *Id*. 790.03 (LexisNexis 2010).

[70] *Royal Globe Ins. Co. v. Superior Court*, 23 Cal.3d 880, 892 (1979) (RICHARDSON, J., concurring and dissenting).

[71] Cal. Ins. Code 790.03 (LexisNexis 2010).

[72] *Id*. 790.09 (LexisNexis 2009).

[73] 46 Cal. 3d 287, 292 (1988).

[74] *Jenkins v. J.C. Penney Casualty Ins. Co.*, 280 S.E. 2d 252 (W.Va. 1981). See Offices of the Insurance Commissioner (2005) for a study on the development and effects of the *Jenkins* decision.

and Montana in 1983,[75] followed the *Royal Globe* doctrine. The above evidence demonstrates that the original conclusion in *Royal Globe* was surprising and likely "startled the insurance industry" (Casey 1983, 917; Wagenseil 1979, 376).

[75] *Klaudt v. Flink*, 202 Mont. 247 (1983).

3 Economic Analysis

3.1 Rationale for Bad Faith Liability

Economic theory predicts both positive and negative effects of increased bad faith liability. While competitive market forces will usually constrain an insurer from systematically underpaying claims, there are situations when this is not guaranteed. Insurers will rationally trade off the immediate cost savings of a reduction in a claim's payment and the discounted future costs of any negative reputational effects (Tennyson and Asmat, 2010). For smaller claims, the potential gains to an insurer from underpaying a claim may be outweighed by the reputational penalties and the subsequent decline in demand for the insurer's product. For larger claims, however, the potential cost savings from underpaying the claims may outweigh the future loss of demand (Sykes, 1996).

Insurers may also underpay claims as a strategy to discourage claimants' incentives to inflate claims (Bond and Crocker, 1997; Crocker and Morgan, 1998; Crocker and Tennyson, 2002). By developing a settlement strategy that reduces the marginal return of filing a larger claim, an insurer can reduce the incentive of a claimant to invest resources in claims exaggeration/fraud.[76] Such a strategy necessarily underpays some claims. Crocker and Tennyson (2002) find empirical support for this strategy in a large dataset of third-party insurance settlements for automobile accidents. The authors show that for claims where the cost of filing a fraudulent claim is low and thus the potential for fraud is high (e.g., wage loss claims), insurers, on average, pay less at the margin than for claims where fraud is more costly (e.g., claims for only medical expenses).

In the absence of legal sanctions for wrongfully underpaying claims, there ex-

[76] Crocker and Tennyson (2002) develop a formal model of an insurers' optimal claims settlement strategy, which involves the systematic underpaying of claims to deter loss exaggeration. In their model, the extent of underpayment is limited by the potential litigation costs and bad faith liability resulting from claims underpayment.

ists an externality affecting insurers' adoption of claims settlement strategies. Specifically, the insurer only considers the benefits of claims underpayment and does not consider the costs to other parties. If there are legal sanctions against the insurer, however, which accurately reflect the costs incurred by the underpaid claimant, then these sanctions will cause the insurer to "internalize[] both the benefits and costs of aggressive claims settlement strategies" (Crocker and Tennyson, 2002, 504-505). Therefore, bad faith liability can result in an efficient balance between the costs of claims underpayment and the benefits from reduced claims fraud (Abraham, 1986; Crocker and Tennyson, 2002; Tennyson and Warfel, 2009).

Bad faith liability, a tort, which includes potential punitive damages, may only be efficient, however, in specific instances. In general, insureds who are underpaid (or denied payment) for legitimate claims, know that they were underpaid, can sue for breach of contract, and have some certainty that the compensatory damages will be calculated with a fair degree of accuracy (see Posner, 2011, 322-323). The necessity for punitive damages may therefore be rare, but "rare is not never" (Posner, 2006, 746). With regard to insurance contracts, the most applicable situation where punitive damages may be efficient is for small claims.[77] If insurers systematically underpay some small claims, then the "aggregate damages may be significant, [but] no single victim of the wrongful act has sufficient damages to make suing worthwhile" (Posner, 2006, 746).[78] By increasing the expected value of the damages, bad faith liability may provide additional protection for smaller claims by giving the claimants an incentive to sue if the insurer underpays the claim.

[77]For a discussion of other situations where punitive damages may or may not be efficient, see Posner (2006, 745-747) and Posner (2011, 320-323).

[78]If punitive damages are not allowed, the parties to small contracts may be compensated in the price for not having adequate legal remedies incase of a breach (Posner, 2006, 746).

3.2 Potential Unintended Consequences of Bad Faith Liability

Despite the potential efficiency enhancing effects of bad faith liability, the threat of increased liability can substantially distort claims settlement negotiations (Sykes, 1996).[79] The implications of insurance bad faith liability become less clear when considering that not all claims that insurers underpay are legitimate. In other words, insurers do and should question "reasonably disputable claims" (Abraham, 1986). It is therefore not the case, as some argue, that "there are no efficient breaches in the insurance context; breaches of insurance contracts can only be opportunistic" (Capozzola, 2000, 196). There are sometimes legitimate factual and legal disputes (Posner 2011, 323; also see Sykes 1996).

Extending bad faith liability significantly increases the potential costs of litigation to insurers, which increases the pressure on insurers to pay reasonably disputable claims. If the expected costs of litigation to insurers significantly increases relative to the benefits of investigations and audits, then insurers will have less of an incentive to invest in these fraud reduction strategies. For example, conducting an investigation on a suspicious claim will delay the time until the claim gets paid and may result in a bad faith suit, which, win or lose, the insurer has to pay to defend against. Specifically, this threat of increased bad faith liability may lead to less claim investigations then there should efficiently be (Abraham, 1986; Tennyson and Warfel, 2009).

Further, the distortionary effects of increased bad faith liability may be more severe in the long run than in the short run. The cost savings that insurers receive from fraud detection strategies may mainly come from deterrence effects, rather than the identification of specific instances of fraud. Implementing fraud detection strategies increases the costs of claimants committing fraud and increases the

[79] See Cooter and Rubinfeld (1989) for a comprehensive overview of the litigation settlement literature.

probability that those claimants are identified and punished.[80] By reducing the incentives of insurers to use fraud detection strategies, however, increased bad faith increases the expected return on fraudulent claims and thus incentivizes claimants to engage in fraud (Picard, 2000; Tennyson and Warfel, 2009).

[80]The potential legal penalties for entering fraudulent claims can be severe. Derrig and Zicko (2002) analyzed the 6,684 (combined to 3,349) automobile and workers' compensation claims that were referred to and accepted by the Insurance Fraud Bureau of Massachusetts between 1991 and 2000. The authors found that 552 cases were referred to prosecution with 84 percent resulting in guilty or equivalent verdicts. Of the cases resulting in a guilty or equivalent verdict, 44 percent received jail sentences, 62 percent received probation and 58 percent received restitution.

4 Research Design

4.1 Relationship to Existing Literature

There are only a few studies that have tried to empirically evaluate the effects of increased insurance bad faith liability on claims settlements (Hawken et al., 2001; Browne et al., 2004; Tennyson and Warfel, 2009; Hyman et al., 2011; Tennyson and Asmat, 2010). Most of these studies are cross-sectional in nature and do not address a "causal" link between increased insurance bad faith liability and particular claims settlement characteristics and/or practices (Browne et al., 2004; Tennyson and Warfel, 2009; Hyman et al., 2011). Further, many of the studies focus on only one or a few claims settlement characteristics, which precludes an analysis on the effects of increased bad faith liability on net social welfare (Hawken et al., 2001; Browne et al., 2004; Tennyson and Warfel, 2009; Hyman et al., 2011; Tennyson and Asmat, 2010).

First, based on an anecdotal analysis of case law, Sykes (1996) concludes that there may be substantial problems with courts applying extracontractual bad faith liability. Specifically, Sykes (1996) finds examples of insurers being found liable for bad faith for refusing to pay claims that were potentially fraudulent,[81] for arguably unnecessary treatments,[82] and possibly outside the scope of the insurance contract.[83] For example, in *Frommoethelydo v. Fire Insurance Exchange*,[84] the insurer refused to pay a claim for stolen property because the insurer believed it

[81]See *T.D.S. Inc. v. Shelby Mutual Insurance Co.*, 760 F.2d 1520 (11th Cir. 1985) (the insurer was found liable for bad faith for refusing to pay a claim where there was evidence that the insured committed arson on his own property).

[82]See *Aetna life Insurance Co. v. Lavoie*, 470 So. 2d 1060 (Ala. 1984) (the insurer was found liable for bad faith for refusing to pay a claim for some of the insureds treatments that the insurers experts believed were not "necessary" or "reasonable.").

[83]See *Sparks v. Republic National Life Insurance Co.*, 647 P. 2d 1127 (Ariz. 1982) (the insurer was found liable for bad faith for refusing to pay a claim where it was not clear in the contract if the insured was covered for treatments after he stopped paying for insurance but for injuries sustained when he was still insured).

[84]42 Cal. 3d 208 (1986).

was fraudulent. At trial, the insurer noted that the claim was the insured's second claim in less than a year, experts believed that the receipt presented for $3,000 of audio/video equipment was altered, and a store clerk testified that the plaintiff asked him to backdate the receipt for the audio/video equipment. Despite these facts, a jury still found the insurer liable for less than $9,000 for the stolen property claim, $250,000 for emotional distress, and $1.25 million for punitive damages (the California Supreme Court reversed all damages except for the property claim and ordered a new trial on the damages question). Based on his analysis, Sykes (1996, 405) concluded that the "remedy" of bad faith liability "may be worse than the problem, as the courts seem to find bad faith on the part of insurers who have genuine and reasonable disputes with their policyholders over the terms of the policy or over factual issues essential to the insureds right to recover."

Moving on to a more empirical analysis, Hawken et al. (2001), like this paper, study the effects of the *Royal Globe* doctrine on automobile injury claims. For the first part of their analysis the authors rely on aggregate statewide data to show that the number of third-party automobile injury claims increased and decreased, relative to other states, with the adoption and future rejection of the *Royal Globe* doctrine. Unlike this paper, however, for the second part of their analysis the authors do not focus on comparing claims characteristics in California to other states before and after the adoption of *Royal Globe*. Instead, the authors compare claims characteristics in California to other states before and after the *Royal Globe* doctrine was overturned in *Moradi-Shalal*. The authors found that compensation payments in California were 26 percent higher than compensation payments in other states when *Royal Globe* was in effect but that trend reversed when *Royal Globe* was overturned.

While the authors did rely on a difference-in-differences design to attempt to draw a causal connection between the overruling of *Royal Globe* and claims settle-

ment characteristics, there was a significant confounding event. In the same year that *Royal Globe* was overturned, California adopted Proposition 103, which made major changes to insurance rate regulation in the state (see e.g., Shelor and Cross 1990; Sugarman 1990; Szewczyk and Varma 1990; Fields et al. 1990). The Proposition required, among other mandates, that every insurer reduce its rates by at least 20 percent,[85] limited the amount of criteria insurers could use to create automobile insurance rates,[86] and required preapproval of rates.[87] The potential confounding effects from Proposition 103 make it difficult to disentangle the effects from the overturning of the *Royal Globe* doctrine that Hawken et al. (2001) found, from the effects of the changes in regulation (Tennyson and Asmat, 2010).

In addition, further empirical analyses of insurance claims settlements shows that increased first-party insurance bad faith liability is associated with higher claims settlements, an increased prevalence of fraud "red flags," and less insurance company investigations. Browne et al. (2004) utilize a large dataset of first-party automobile insurance claims settled in 38 states in 1992. After controlling for multiple factors that are expected to be associated with the size of settlement payments, the authors find that claim settlements are higher in states that allow for private actions for insurer bad faith (for both the economic and non-economic portions of the settlements). Further, Tennyson and Warfel (2009) examine first-party automobile insurance claims settled in 1997 to show that bad faith liability is associated with more fraud "red flags" (e.g., lack of a police report, only or primarily a sprain injury, etc.)[88] being settled and less investigations (e.g., medical audits and independent medical exams) by insurance companies. While Browne et al. (2004) and Tennyson and Warfel (2009) do shed valuable light onto questions regarding

[85]Cal. Ins. Code 186.01 (a) (LexisNexis 2010).
[86]*Id*. 186.02 (a) (LexisNexis 2010).
[87]*Id*. 186.01 (c) (LexisNexis 2010).
[88]Crocker and Tennyson (2002, 469) show that insurer underpayment of claims is associated with claims for which "loss exaggeration is easier."

bad faith liability, both studies show associations and do not address a causal link between claim characteristics and bad faith liability.[89]

Finally, Tennyson and Asmat (2010)[90] attempt to draw the first causal connection between first-party insurance bad faith liability and claims characteristics. The authors rely on cross-state and over-time variation in state bad faith regimes while utilizing a large dataset of first-party automobile insurance claims settled in 42 states in 1977, 1987, and 1997. Consistent with their hypotheses, the authors show that adopting insurer bad faith liability increases claim settlements and decreases the probability of claims being underpaid. Further, they find that the impact of tort liability on settlement amounts is greater for smaller claims. Based on these findings, however, "normative inferences regarding the consequences of bad faith laws on claim settlements are not possible" (Tennyson and Asmat, 2010, 26). As the authors note, higher claims settlements may be evidence of insurers paying legitimate claims that they would have otherwise underpaid or evidence of insurers overpaying claims (potentially fraudulent claims).

Due to the lack of evidence on the causal effect of increased bad faith liability on net social welfare, Abraham (1994); Jerry (1994); Tennyson and Warfel (2009); Tennyson and Asmat (2010) noted the need for additional studies. This study uses the increase in third-party insurance bad faith liability faced by insurers in only California, created by the *Royal Globe* doctrine, to provide causal estimates of the effects of increased bad faith liability on a wide range of closed claim outcomes.

[89]Hyman et al. (2011) also empirically evaluate an association between bad faith liability and claims characteristics. Using data from Texas for 1988-2005 on closed, commercially insured personal injury claims, the authors show that most claims are settled at or close to the policy limit. They also demonstrate that claims settled at policy limits, settle quicker.

[90]See Asmat (2009) for a similar analysis.

4.2 Hypotheses

This study aims to add to the current debate by examining closed claim outcomes related to the welfare implications of insurance bad faith liability. As discussed above, the previous literature is mostly associational and only focuses on one or a few aspects of claims characteristics or settlement practices. This precludes making a welfare argument regarding bad faith liability. Increased claims settlements, for example, may be a result of insurers not underpaying legitimate claims (efficiency enhancing) or overpaying illegitimate claims (efficiency diminishing). This study, therefore analyzes various aspects of claims characteristics and settlement practices to paint a fuller picture of the effects of bad faith liability on net social welfare. Specifically, I look at the effects of increased third-party insurance liability in California before and after *Royal Globe*, relative to a control group of states, on the average compensation paid for third-party automobile bodily injury (BI) claims, the average time period until a settlement is made for BI claims, the frequency of fraud red flags in settled BI claims, and the rate of insurance company investigations for settled BI claims.

The characteristics of the data influence the specific hypotheses that I will test.[91] First, the data only contains automobile insurance claims that were settled for some amount. Therefore, it is not practical to test for the frequency or severity of BI claims. Second, the data does not have information on the bargaining demands of the injured parties for non-economic damages, such as mental distress. The data is, however, well suited to test the hypotheses that increased third-party insurance bad faith liability from *Royal Globe* causes an increase in special damages paid, an increase in total damages paid, a decrease in the level of undercompensation, a decrease in the time between the claim and the first payment, an increase in the

[91] Similar data is used by Crocker and Tennyson (2002); Browne and Schmit (2008); Tennyson and Warfel (2009) and Tennyson and Asmat (2010).

number of fraud red flags, and a decrease in the number of insurance company physical investigations for closed claims in California, relative to the same outcomes for closed claims in a control group of states.

In total, I test the following specific hypotheses:

H1: For similar settled BI claims, the average specials paid in California will increase relative to the control group of states.

H2: For similar settled BI claims, the average total compensation paid in California will increase relative to the control group of states.

H3: For similar settled BI claims, the percent of claims that are under-compensated will decrease in California relative to the control group of states.

H4: For similar settled BI claims, the average length of time between the claim being filed and the first payment will decrease in California relative to the control group of states.

H5: For similar settled BI claims, the average number of fraud red flags will increase in California relative to the control group of states.

H6: For similar settled BI claims, the average number of physical examinations will decrease in California relative to the control group of states.

The relative magnitudes of hypotheses *H1* to *H6* may shed light on the impact on net social welfare of increased bad faith liability. If, for example, the average compensation paid increases, the average time before the first payment decreases, and the number of fraud red flags does not change (or increase to a similar magnitude) for similar closed claims in California relative to the control group of states, then the increase in bad faith liability may be efficiency enhancing.

4.3 Data and Variables

To test the above hypotheses I rely on a countrywide sample of administrative claims data for closed bodily injury liability claims collected by the Insurance Re-

search Council (IRC).[92] Bodily injury liability insurance covers "insured's legal liability if he causes bodily injury to someone else through the ownership, maintenance or use of the vehicle, up to policy limit specified" (All-Industry Research Advisory Committee, 1979, 8). The participating insurers completed an extensive questionnaire for every claim closed during a two week period in 1977 and in 1987. The 1977 and 1987 data were obtained from 29 and 34 leading auto insurers and include 21,885 and 21,584 bodily injury claims, respectively. Collectively, the participating insurers represented about 60 percent of the countrywide volume of private passenger automobile insurance written in the U.S. at the time they were sampled.[93] Most claims are from accidents occurring in 1976-1977 and 1986-1987, but accidents extend from 1970-1977 and 1980-1987. All claimed losses are converted to 1987 dollars. From the data, I focus on the following closed claim outcomes:

Specials paid: The compensation (in $10,000) for "[s]pecific expenses incurred by claimant as a result of the accident" (All-Industry Research Advisory Committee, 1979, 8).[94] To adjust for inflation, the amounts for 1977 closed claims are converted to 1987 dollars.[95]

[92]The IRC (previously the All-Industry Research Advisory Council ("AIRAC")) is an independent, not-for-profit organization supported by leading property-casualty insurance organizations. Its mission is to provide "timely and reliable empirical research to all parties involved in public policy issues affecting risk and insurance. It does not advocate public policy or directly influence specific legislative initiatives or engage in lobbying communications." (http://www.ircweb.org/)

[93]To increase response rates, each insurer was allowed to select the two week period (10 consecutive work days in 1977) that they were going to respond for. In 1977, insurers were allowed to select a time period between October 2nd and November 20th. In 1987, most of the sampling took place in May, June and July. See All-Industry Research Advisory Committee (1979) and All-Industry Research Advisory Council (1989) for a more detailed description of the data.

[94]For claims closed in 1977, specials paid is calculated as the amount reimbursed for emergency treatment + outpatient diagnostic exams + outpatient drugs + x-rays + physical therapy + inpatient hospital charges + physician + all other medical + wage loss + essential services + rehabilitation + funeral expenses (see All-Industry Research Advisory Committee, 1979, 159). For claims closed in 1987, specials paid is calculated as the amount reimbursed for total medical expenses + wage loss + expenses for replacement services + rehabilitation expenses + other expenses (see All-Industry Research Advisory Council, 1989, 164).

[95]All conversions for inflation are done using the CPI Inflation Calculator provided by the Bureau of Labor Statistics, available at http://www.bls.gov/data/inflation_calculator.htm (accessed February 20, 2011). One dollar in 1977 had the same purchasing power as $1.8746 in 1987.

Total paid: The total of all amounts paid (in $10,000) to the claimant in settlement of the claim. This amount is equal to the specials paid plus the general damages paid. General damages paid are the compensation for "infringement on civil rights in excess of economic loss. Includes such things as past or future inconvenience, continuing disability and/or disfigurement" (All-Industry Research Advisory Committee, 1979, 8). To adjust for inflation, the amounts for 1977 closed claims are converted to 1987 dollars.

Undercompensation: A binary variable, equal to one if there were any economic losses uncompensated for, or equal to zero if the economic losses were fully compensated or if the only reason for the economic losses being under-compensated is that the claimed loss exceeded the policy limits.

Days until first payment: The number of days from the day of the accident to the day the first payment was made.

Fraud red flags: The number of fraud red flags appearing in a closed claim. Following Tennyson and Warfel (2009, 227), the fraud red flags used are that the injury report was received after the accident report and that only a sprain/strain injury.[96] The lack of a noticeable injury at the time of the accident makes it more likely that a claimed injury is exaggerated or fictitious. It is possible that a victim did not notice his/her injury on the day of the accident but it is increasingly unlikely. Further, soft tissue injuries are difficult to medically verify and are "prone to falsification and exaggeration" (Tennyson and Warfel 2009, 227; Dionne and St-Michel 1991, 238-239). For this reason, only sprain/strain injuries reported are used as fraud red flags. This variable is standardized between zero and one.

Physical examinations: A binary variable, equal to one if the insurer required a physical examination, and equal to zero if the insurer did not require a physical examination.

[96]See Weisberg and Derrig (1991, 1996) for a discussion of indicators of potential fraud.

4.4 Empirical Methods

To formally test each of the above hypotheses I utilize a difference-in-differences ("DD") procedure. In DD studies, researchers examine the progression of an outcome (e.g., unemployment rates, literacy rates, etc.) for a unit affected by a particular event (usually a law) and compare it to the progression of the same outcome for a "control" group of units unaffected by the event. DD studies are widely used by social scientists (Bertrand et al., 2004; Puhani, 2008; Meyer, 1995; Athey and Imbens, 2006).

Snow (1855), for example, was probably the first researcher to use such a method (Angrist and Pischke, 2009, 227-228). Snow showed that cholera was transmitted in London through contaminated drinking water by comparing changes in death rates between two water companies. In 1849, both companies obtained their water from the dirty Thames River in central London and their customers had similar death rates. In 1852, however, one of the companies moved their water intake to a relatively clean position up stream and its customers realized a drop in death rates relative to the alternate company. Card and Krueger (1994), in a well known study, analyze the employment between a sample of fast food restaurants in New Jersey and Pennsylvania before and after the New Jersey minimum wage law went into effect. Further, Pischke (2007) estimates the effects of changes in school term length on student performance by utilizing changes in policy in some German counties, relative to Bavaria (the control), which experienced no policy change.

In order for a DD study to produce an unbiased estimate of the effect of the intervention, it is necessary that the intervention is "as good as random" (Bertrand et al., 2004, 250). As Besley and Case (2000) note, however, the adoption of policies can be endogenous. For example, "economic conditions that brought about [a] policy change may have independent effects on the outcome variable of interest" (Besley and Case, 2000, F688). Fortunately, the *Royal Globe* decision does not appear

to suffer from potential endogeneity. As discussed above, the decision was not in response to a change in law or societal factors but the result of the Court's different interpretation of the same law. The decision was "unprecedented" and "startled the insurance industry" (Casey 1983, 917; Wagenseil 1979, 376; Aitken and Abeltin 1987, 55).

In addition to the exogenous intervention requirement, it is also necessary for the control group of units to be an adequate counterfactual to the treated unit (i.e., represent what the outcomes of interest in California would have been had it not been for the *Royal Globe* decision). "The question of whether this comparison is a good one deserves careful consideration" (Angrist and Pischke, 2009, 241). In DD studies there is usually some "degree of ambiguity about how comparison units are chosen" (Abadie et al., 2010, 493). As Abadie et al. (2010, 493-494) argue, traditional regression based methods are susceptible to two main shortcomings. First, the lack of a systematic way of choosing controls removes transparency by allowing researchers to select controls while seeing how those decisions affect the outcome. Second, it is often challenging to pick a control to adequately approximate the treated unit. Difficulties arise "if pre-treatment characteristics that are thought to be associated with the dynamics of the outcome variable are unbalanced between the treated and the untreated group" (Abadie, 2005, 2).

Even while controlling for observed individual claim characteristics and state fixed effects, it may be too strong of an assumption to assume that the most appropriate control for California is all of the remaining states (see Fitzpatrick, 2008, 21). It is more likely that California (or any other state) will trend more closely with states that have similar characteristics. In other words, it is more likely that unobserved characteristics in California would have trended similarly, but for the treatment (*Royal Globe*), to unobserved characteristics in states that resemble California than in very different states. Abadie and Gardeazabal (2003), Fitzpatrick

(2008), and Abadie et al. (2010) all show that similar states (created using the synthetic control procedure defined below) trend better with each other than when compared to a national average.

I therefore follow Abadie and Gardeazabal (2003) and Abadie et al. (2010) by using a "data-driven procedure" to select an adequate counterfactual by designing a "synthetic control" of multiple units (the Abadie-Diamond-Hainmueller method).[97] The general idea is that a combination of units usually provides a better comparison to the one treated unit than just a single unit would (Abadie and Gardeazabal, 2003; Abadie et al., 2010). Abadie and Gardeazabal (2003), for example, develop a synthetic unit made up of two Spanish regions to approximate the economic growth that the Basque country would have experienced, but for terrorist activity. While Abadie et al. (2010) use five states to construct a synthetic California to estimate the per-capita cigarette sales that California would have experienced had Proposition 99 not been implemented. In both studies the authors demonstrate that the "synthetic control" they create trends with the treated unit better than a national average or an average of all of the potential control units (Abadie and Gardeazabal 2003, 113; Abadie et al. 2010, 499-500).

Many other researchers have implemented the Abadia-Diamond-Hainmueller method for a wide range of applications. Fitzpatrick (2008) examines the effects of universal pre-kindergarten, Groen and Polivka (2008) study the effect of Hurricane Katrina on the labor market outcomes for evacuees, Nannicini and Billmeier (2011) study the effects of trade openness on economic growth, Forlani (2009) analyzes the efficiency changes in Irish firms' productivity due to variations in input composition, Trandafir (2009) examines the effects on different-sex marriages after the passage of same sex marriage laws in the Netherlands, and Keele (2009) studies the effects of ballot initiatives on state voter turnout and fiscal policy. The strengths

[97] See Imbens and Wooldridge (2009) for an additional description of the Abadie-Diamond-Hainmueller method and a discussion of the new developments in the causal evaluation of policies.

and versatility of the synthetic control method make it well suited to apply to this study's setup and data.

As this study aggregates data at the state level, in order to avoid using small sample sizes, states that have less than 35 total observations are dropped.[98] In addition, to avoid comparing states that would likely not trend in the same way as California, but for *Royal Globe* (e.g., states that experienced a change in a relevant law, states that are significantly different from California, etc.), observations from states that use no fault liability,[99] changed their third-party insurance bad faith liability laws,[100] adopted a private cause of action under an unfair claim settlement practices statute,[101] and/or implemented a *Royal Globe* type decision[102] during the time period of interest (1975-1987) are removed. I restrict the sample to obser-

[98]I only consider claims with a positive claimed economic loss.

[99]States that used no fault liability between 1975-1987: Colorado; Connecticut; District of Columbia; Florida; Georgia; Hawaii; Kansas; Kentucky; Massachusetts; Michigan; Minnesota; New Jersey; New York; North Dakota; Pennsylvania; and Utah (All-Industry Research Advisory Committee 1979, 9; All-Industry Research Advisory Council 1989, 136-148.

[100]States that changed their third-party insurance bad faith laws between 1975-1987: Alaska (*Continental Ins. Co. v. Bayless Roberts, Inc.*, 608 P2d 281 (Alaska 1980)); Colorado (*Farmers Group, Inc. v. Trimble*, 691 P2d 1138 (Colo 1984)); Florida (*Boston Old Colony Ins. Co. v. Gutierrez*, 382 So 2d 783 (Fla 1980)); Georgia (*McCall v. Allstate Ins. Co.*, 251 Ga 869 (1984)); Illinois (*Edwins v. General Cas. Co.*, 78 Ill App 3d 965 (1979)); Iowa (*Kooyman v. Farm Bureau Mut. Ins. Co.*, 315 NW2d 30 (Iowa 1982)); Michigan (*Commercial Union Ins. Co. v. Liberty Mut. Ins. Co.*, 137 Mich App 381 (1984)); Minnesota (*Short v. Dairyland Ins. Co.*, 334 NW2d 384 (Minn 1983)); Montana (*Gibson v. Western Fire Ins. Co.*, 682 P2d 725 (Mont 1984)); Nebraska (*Hadenfeld v. State Farm Mut. Automobile Ins. Co.*, 195 Neb 578 (1976)); New Jersey (*Fireman's Fund Ins. Co. v. Security Ins. Co. of Hartford*, 72 NJ 63 (1976)); New Mexico (*Ambassador Ins. Co. v. St. Paul Fire & Marine Ins. Co.*, 102 NM 28 (1984)); Oregon (*Maine Bonding & Casualty Co. v. Centennial Ins. Co.*, 298 Or 514 (1985)); Rhode Island (*Voccio v. Reliance Ins. Co.*, 703 F2d 1 (Ca1 1983)); and Vermont (*Myers v. Ambassador Ins. Co.*, 508 A2d 689 (Vt 1986)). See Ashley (1987, 89-134).

[101]States whose courts recognized a private cause of action arising from an unfair claims settlement practices statute between 1975-1987: Arizona (*Sparks v. Republic National Life Ins. Co.*, 132 Ariz 529 (1982)); Connecticut (*Griswold v. Union Labor Life Ins. Co.*, 186 Conn 507 (1982)); Florida (Fla Stat Ann 624.155); Massachusetts (Mass Ann Laws ch 176D, 3(9); *Whitney v. Continental Ins. Co.*, 595 F Supp 939 (D Mass 1984)); Montana (Mont Code Ann 33-18-201; *Klaudt v. Flink*, 658 P2d 1065 (Mont 1983)); North Dakota (ND Cent Code 26.'-04-03(9); *Farmer's Union Cent. Exch., Inc. v. Reliance Ins. Co.*, 626 F Supp 583 (D ND 1985)); Rhode Island (RI Gen Laws 9-1-33); Virginia (Va Code 38.'-15.9; *Morgan v. American Family Life Ins. Co.*, 559 F Supp 477 (WD Va 1983)); and West Virginia (W Va Code 33-11-4(9); *Jenkins v. J.C. Penney Cas. Ins. Co.*, 280 SE2d 252 (W Va 1981). See Ashley (1987, 89-134).

[102]States that implemented a *Royal Globe* type decision between 1975-1987: Montana (*Klaudt v. Flink*, 202 Mont. 247 (1983)); and West Virginia (*Jenkins v. J.C. Penney Casualty Ins. Co.*, 280 S.E. 2d 252 (W.Va. 1981)). See Stempel (2006, 9.05-9.06).

Table 1: Closed Claims by Year Closed and Accident Year

	Year Closed		
Accident Year	1977	1987	Total
1975	430	0	430
1976	1,135	0	1,135
1977	3,132	0	3,132
1985	0	1,171	1,171
1987	0	5,585	5,585
1986	0	4,530	4,530
Total	4,697	11,286	15,983

vations from 1975-1977 and 1985-1987 to allow for states that changed their bad faith liability laws between 1970-1974 to be included (there are only a few observations between 1970-1974 and 1980-1984). Appendix Table A.1 shows the bad faith liability and general terms of liability for automobile accidents in effect in states between 1970 and 1987. Table 1 shows the number of remaining closed claims by the accident year and the year the claim was closed.

To produce a synthetic California that approximates how insurers in California would have settled claims had it not been for the *Royal Globe* decision, it is necessary to construct a synthetic that treats pre-*Royal Globe* claims with similar characteristics in similar ways (i.e., create a synthetic state where insurers receive similar claims and use a similar indemnification strategy to California). This is necessary because insurers settle claims with different characteristics from different states in different ways (Hawken et al., 2001; Crocker and Tennyson, 2002; Browne et al., 2004; Tennyson and Warfel, 2009; Hyman et al., 2011; Tennyson and Asmat, 2010). To do this, and to get a sense of potential heterogeneous treatment effects, I conduct the analysis by quantiles of the expected claimed loss.[103]

[103]Buchmueller et al. (2009), for example, divide observations into quantiles of the probability of receiving health benefits in a voluntary market, to examine potential heterogeneous effects of Hawaii's Prepaid Health Care Act. Additionally, Card (1993, 1995), divides observations into quar-

Let C_{ist} be the claimed loss for claimant i in state s in time of claim closure t, and $\mathbf{O}_{ist} = (o_{1ist}, ..., o_{mist})$ be a vector of m observable claims characteristics (i.e., the claims characteristics in column 1 of Table 2). To group claims with similar characteristics together, an OLS regression is used to calculate the expected claimed loss for pre-*Royal Globe* claims (i.e., claims closed in 1977), $L_0 = E[C_{ist}|O_{i,s,t=1977}]$. The expected claimed loss for pre-*Royal Globe* claims, L_0, is equally divided into Q quantiles, $q = 1, ..., Q$, with $\mathbf{A}$ = (a$_1, \ldots, a_5$) representing a vector of the maximum L_0 value in each quantile q (i.e., the values in column 4 of Table 3). Table 2 shows the OLS results and Table 3 shows the range of values in each quantile q.

Once similar pre-*Royal Globe* claims are grouped together (into quantiles q), weighted combinations of J potential control states are chosen to create synthetic Californias that best resemble the expected value for each of K closed claim outcomes (e.g., specials paid, fraud red-flags, etc.), labeled $k = 1, \ldots, K$, inside of each quantile q in California before *Royal Globe* and the expected value of each of M closed claim characteristics (e.g., sprain injury), labeled $m = 1, ..., M$, inside of California before *Royal Globe*. Let $\mathbf{W} = (w_1, \ldots, w_J)'$, a $(J \times 1)$ vector where the scalar w_j represents the weight of state j in the synthetic California. Let the scalar $y_{k,q}$ represent the expected value of a closed claim outcome k in quantile q in California pre-*Royal Globe*. That is $y_{k,q}$ = E[k$_{istq}|, s = California, t = 1977, q = z](z = 1, \ldots, Q)$. Let the scalar m represent the expected value of each claim characteristic m_m in California pre-*Royal Globe*. That is $m_m = E[m_{istq}|s = California, t = 1977]$. Then let $\mathbf{Y}_k = (y_{k,1}, y_{k,2}, \ldots, y_{k,Q}, m_1, \ldots, m_M)'$ as a $(QM \times 1)$ vector and let $\mathbf{X_{k,j}}$ be a $(QM \times J)$ matrix of the same values for the J potential control states.

Then, using the Abadie-Diamond-Hainmueller method and the "Synth" Stata program developed by Abadie, Diamond, and Hainmueller,[104] (see Abadie and

tiles of predicted education to compare the mean levels of education for men who grew up in areas with or without a local college.

[104]The "Synth" program for Stata, MATLAB, and R is available on Jen Hainmueller's webpage at http://www.mit.edu/ jhainm/software.htm (accessed February 16, 2011).

Table 2: OLS Estimation: ln(Claimed Loss)

Variable	Coef.	(Std. Err.)
Acc. Location: Central City	0.049	(0.056)
Acc. Location: Medium City	-0.084	(0.061)
Acc. Location: Small Town	-0.126 *	(0.067)
Acc. Location: Rural	-0.151 **	(0.076)
Claimant is Driver	0.100	(0.109)
Claimant is Passenger	0.013	(0.110)
Claimant is Other	0.357	(0.299)
Number of Other Vehicles in Accident	0.035	(0.032)
Male Claimant	0.317 ***	(0.039)
Claimant's Age	0.014 ***	(0.001)
Extent of Disability: Temporary	0.951 ***	(0.179)
Extent of Disability: Permanent Partial	0.950 ***	(0.286)
Extent of Disability: Permanent Total	1.553 *	(0.869)
Extent of Disability: Fatal	3.155 ***	(0.218)
Injury: Sprain	0.413 ***	(0.052)
Injury: Fracture	0.716 ***	(0.073)
Injury: Laceration	0.028	(0.067)
Injury: Other	0.013	(0.045)
Inpatient Hospitalization: 1 Night	0.488 ***	(0.163)
Inpatient Hospitalization: 2-7 Days	0.903 ***	(0.079)
Inpatient Hospitalization: Over 7 Days	1.475 ***	(0.085)
Wage Loss Claimed	1.275 ***	(0.153)
Medical Loss Claimed	1.305 ***	(0.150)
Other Expenses Claimed	0.609 ***	(0.108)
Ext. of Dis: Temp. x Wage Loss Claimed	-0.241	(0.176)
Ext. of Dis: Temp. x Medical Loss Claimed	-0.319 *	(0.166)
Ext. of Dis: Perm. Part. x Wage Loss Claimed	0.367	(0.278)
Ext. of Dis: Perm. Part. x Medical Loss Claimed	0.368	(0.250)
Ext. of Dis: Perm. Tot. x Wage Loss Claimed	0.012	(0.858)
Ext. of Dis: Perm. Tot. x Medical Loss Claimed	0.843	(0.740)
Ext. of Dis: Fatal x Wage Loss Claimed	0.790	(1.240)
Ext. of Dis: Fatal x Medical Loss Claimed	-0.678	(0.540)
Constant	3.127 ***	(0.202)
N	4,292	
R^2	0.367	

Note: Omitted variables include: Acc. Location: Suburb; Claimant is Pedestrian; Extent of Disability: and None; Inpatient Hospitalization: None. *** indicates statistical significance at the 1 percent confidence level; ** indicates significance at the 5 percent confidence level; and * indicates significance at the 10 percent confidence level.

Table 3: Predicted Claimed Loss by Quantiles
(ln(claimed loss))

Quantile	Mean	Min.	Max.
1	5.304	3.886	5.704
2	5.908	5.705	6.071
3	6.232	6.072	6.379
4	6.591	6.379	6.927
5	7.820	6.929	11.092

Note: The table shows the mean predicted claimed loss by quantile. The minimum and maximum predicted claimed loss are also reported by quantile.

Gardeazabal (2003) and Abadie et al. (2010) for a detailed discussion), I construct a synthetic California for each closed claim outcome k. The synthetic is created by choosing the weights, $\mathbf{W}^*$, to minimize the distance

$$||\mathbf{Y} - \mathbf{XW}||\mathbf{V} = \sqrt{(\mathbf{Y} - \mathbf{XW})'\mathbf{V}(\mathbf{Y} - \mathbf{XW})}, \tag{1}$$

between $\mathbf{Y}$ and $\mathbf{XW}$, where $\mathbf{V}$, a $(QM \times QM)$ symmetric and positive semidefinite matrix, assigns weights to linear combinations of the variables in $\mathbf{Y}$ and $\mathbf{X}$. Following Abadie et al. (2010, 496), who build on Abadie and Gardeazabal (2003), $\mathbf{V}$ is selected "among positive definite and diagonal matrices such that the mean squared prediction error [(MSPE), which is the average of the squared discrepancies between California and the synthetic California of the outcome variable,] is minimized for the preintervention periods." Further, to prevent extrapolation (see Abadie and Gardeazabal 2003, 117n; Abadie et al. 2010, 496), the procedure to select weights $\mathbf{W}^*$ is subject to the constraints: $w_j \geq 0 (j = 1, 2, \ldots, J)$ and $w_1 + \ldots + w_J = 1$.[105]

The optimal weights, $\mathbf{W}^*$, for each closed claim outcome are shown in Table 4. Figure 1 shows the closed claim outcomes for California and synthetic Califor-

[105]These constraints restrict the states included in the synthetic control to states that are similar to California. If w_j could be less than zero, then $\mathbf{W}^*$ could include weights that assign "penalty terms" to states that are substantially different than California (Abadie et al., 2010, 496).

nia for claims closed in 1977. For each closed claim outcome pre-*Royal Globe*, the synthetic California closely approximates the real California. Table 5 shows that, on average, for each of the closed claim outcomes for each quantile, the synthetic California does a better job representing the real California than does the average of the remaining potential control states. The MSPE, is lower - less than a sixth - for the synthetic California than for the average of the potential control states.

Subsequently, to analyze the changes in California from *Royal Globe*, it is necessary to produce California's counterfactual for closed claims in 1987 (i.e., what the closed claim outcomes would have been in California in 1987, but for *Royal Globe*). To do this, the coefficients from the OLS model shown in Table 2 are applied to the claims closed in 1987 to estimate the predicted claimed loss, had the 1987 closed claims been closed in 1977, $L_1 = E[C_{i,s,t=1987}|O_{i,s,t=1977}]$. Then, to match the 1987 closed claims to similar 1977 closed claims, L_1 is divided into the same quantiles, $q = 1, ..., Q$, as the 1977 closed claims, using the cutoff points $\mathbf{A}$. Appendix Table A.2 shows the mean values for claim characteristics by year closed and by quantile. Using the same approach as the one used for the 1977 closed claims, I calculate the mean for each closed claim outcome, k, in each potential control state, j, in each quantile, q. Finally, the weights in $\mathbf{W}^*$ are applied to the potential control states to produce the synthetic California for closed claims in 1987 (i.e., California's counterfactual).

Then to further test the hypotheses, I run DD OLS regressions on the micro-level data. "Because of the strengths of the synthetic control method," I follow Fitzpatrick (2008, 25)[106] by applying the synthetic control method to the individual-level data. To do this, I assign sample weights to each observation corresponding to the weight for the observation's state determined using the synthetic control

[106]Fitzpatrick (2008) followed the method for creating a synthetic control as defined by Abadie et al. (2007), an earlier version of Abadie et al. (2010).

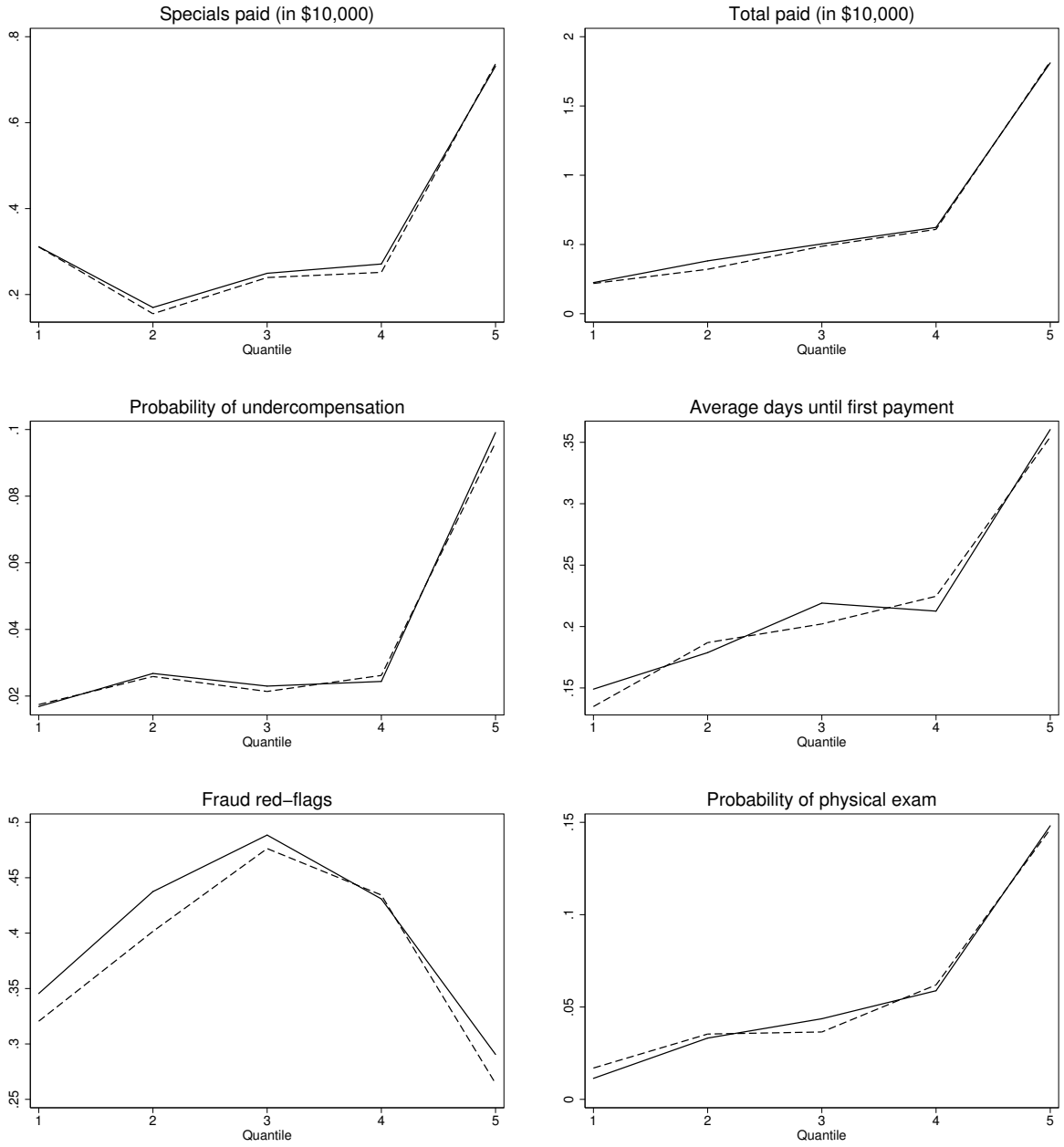

Figure 1: Closed claim outcomes: California vs. synthetic California, 1977

Note: California is solid line. Synthetic California is dashed line.

Table 4: State weights in the synthetic California

State	Specials paid	Total paid	Under-comp.	Physical exam.	Fraud red flags	First payment
Alabama	0	0	.116	0	0	0
Arkansas	0	0	.127	.006	.113	0
Delaware	.156	0	0	0	0	.018
Idaho	.183	.033	0	.047	0	0
Indiana	0	0	.066	0	0	.141
Louisiana	0	.294	0	.057	.192	0
Maine	0	0	0	.078	0	.311
Maryland	0	0	0	0	.377	.531
Mississippi	0	0	0	0	.001	0
Missouri	.08	0	0	0	0	0
Nevada	0	0	.036	.053	0	0
New Hampsire	0	0	.233	0	0	0
North Carolina	0	0	0	0	0	0
Ohio	0	0	0	.643	0	0
Oklahoma	.099	0	0	0	0	0
South Carolina	0	0	0	0	0	0
Tennessee	0	0	0	0	0	0
Texas	0	.274	.421	.032	.317	0
Washington	0	.078	0	.048	0	0
Wisconsin	.482	.321	0	.036	0	0

Note: The values for specials paid and total paid are in $10,000 dollars. Days until first payment is measured in 1,000 days.

Table 5: Closed Claims Outcome Means

Closed claim outcome	Quantile	California	Synthetic Cal.	National Avg.
Days Until First Payment	(1)	0.149	0.135	0.107
	(2)	0.179	0.187	0.150
	(3)	0.219	0.202	0.170
	(4)	0.213	0.225	0.196
	(5)	0.360	0.354	0.272
Fraud Red Flags	(1)	0.346	0.321	0.292
	(2)	0.438	0.402	0.369
	(3)	0.489	0.476	0.433
	(4)	0.431	0.434	0.359
	(5)	0.291	0.264	0.219
Not Full Compensation	(1)	0.017	0.017	0.017
	(2)	0.027	0.026	0.041
	(3)	0.023	0.021	0.013
	(4)	0.024	0.026	0.020
	(5)	0.099	0.096	0.040
Physical Examinations	(1)	0.011	0.017	0.031
	(2)	0.033	0.035	0.053
	(3)	0.044	0.036	0.058
	(4)	0.059	0.062	0.100
	(5)	0.148	0.146	0.135
Specials Paid	(1)	0.311	0.311	0.222
	(2)	0.170	0.155	0.113
	(3)	0.249	0.240	0.179
	(4)	0.271	0.252	0.216
	(5)	0.731	0.737	0.804
Total Paid	(1)	0.225	0.219	0.139
	(2)	0.382	0.321	0.306
	(3)	0.504	0.487	0.399
	(4)	0.623	0.609	0.522
	(5)	1.811	1.822	2.069
MSPE	-	-	0.000	0.006

method.[107]

Using the sample weights the following DD OLS model is run:

$$k_{ist} = \alpha + \beta(Cal_s \times Post_t)_{st} + \lambda O_{ist} + State_s + \tau Post_t + \epsilon_{ist}. \quad (2)$$

The value k_{ist} represents the closed claim outcome of claim i in state s in time closed t. O_{ist} represents a vector of claim characteristics.[108] State fixed effects are represented by $State_s$. $Post_t$ is a binary variables that take on the value of one if the claim is closed in 1987 and Cal_s is a binary variable that takes on the value of one if the claim is from California. β is therefore the difference-in-differences estimate of the effect of *Royal Globe*.

A simple placebo test shows that the DD OLS model 2, above, even while controlling for individual claims characteristics and for state fixed effects, works better when weighting each observation by the weight for that observation's state as determined by the synthetic control method than by not weighting at all.[109] Using the synthetic control method, described above, I create a synthetic state for each potential control state, apply that synthetic control to the individual-level data by assigning sample weights to each observation corresponding to the weight for the observation's state determined using the synthetic control method, and then run model 2 to calculate the estimated treatment effect for each potential control state. At the 5 percent confidence level, one would expect to find a statistically signifi-

[107]Fitzpatrick (2008) assesses the effects of Universal Pre-Kindergarten ("Pre-K") in Georgia by using individual-level student data from the National Assessment of Educational Progress. To apply the synthetic control method to this individual-level data, Fitzpatrick (2008, 25) multiplies the "the sample weights for each student's observation by the corresponding weight for the student's state of residence from the synthetic control method."

[108]Closed claim characteristics include dummy variables for the type of losses claimed (e.g., medical losses), the location of the accident (e.g., central city), the claimant's status (e.g., driver), the claimant's sex, the extent of disability (e.g., fatality), the type of injuries (e.g., sprain), and time period in the hospital (e.g., 1 night). The log of the claimed loss, the number of vehicles in the accident, and the age of the claimant are also included.

[109]See Bertrand et al. (2004), Cameron et al. (2008), and Buchmueller et al. (2009) for similar placebo tests.

cant result for approximately 5 percent of the placebo states. For all of the outcome variables, Table 6 demonstrates that for the unweighted DD OLS regressions for each potential control state, we can reject the null hypothesis of no effect at rates significantly higher than 5 percent. Using the sampling weights, as calculated via the synthetic control procedure, however, allows us to reject the null hypothesis at the 5 percent confidence level, at rates much closer to 5 percent of the placebo states than the unweighted models (will discuss why the rejection rate is still above 5 percent and problems with clustering below). These results suggest that the weighted models may be relying on a slightly weaker, more realistic assumption, that the synthetic weights, when applied to the micro data, create better approximations of the counterfactuals than all of the potential control states while just controlling for individual claims characteristics. As we still find significant treatment effects, even with weighting, for some outcomes for more than 5 percent of the potential control states, the results, however, also suggest that the weighted DD OLS models may still not be adequately controlling for all confounding effects.

One possible explanation is that the effects of *Royal Globe* may be heterogeneous. That is, *Royal Globe* may affect different subgroups of claims differently (see e.g., Tennyson and Asmat, 2010). In order to test for this possibility, I conduct Chow Tests (Chow, 1960), which test for equality between sets of coefficients in two regressions. If *Royal Globe* affects different subgroups of claims in different ways, then a Chow Test will confirm that the coefficients in regressions run on two different subgroups will be statistically different. The Chow Test has been applied to test for structural differences on the impacts on subgroups in a wide range of situations[110] and due to the large sample size of this data, is well suited for this

[110]See e.g., Crain and Zardkoohi (1978, 401-403) (to test for structural differences in operating costs between public and private firms); Chrisman (1989, 410) (to determine if the data for clients who seek operating and administrative assistance can be pooled when examining the value added by outside consultants to pre-venture entrepreneurs); Lemmink et al. (1998, 171) ("to test whether ... different purposes [for visiting restaurants] would affect dynamics of satisfaction formation"); Schlenker et al. (2005, 399)("... to determine whether all coefficients for the two groups of dryland,

Table 6: Placebo Test

(Rejection rates for weighted vs. unweighted DD OLS regressions at 5 percent confidence level)

Variable	Unweighted	Weighted	Difference
Total paid (in $10,000$)	0.600 ***	0.100	0.500 ***
	(0.112)	(0.069)	(0.136)
Specials paid (in $10,000$)	0.250 *	0.200	0.050
	(0.099)	(0.092)	(0.088)
Fraud red flags	0.650 ***	0.500 ***	0.150 *
	(0.109)	(0.115)	(0.082)
Days until first payment	0.800 ***	0.300 **	0.500 ***
	(0.092)	(0.105)	(0.136)
Physical exam	0.550 ***	0.100	0.450 ***
	(0.114)	(0.069)	(0.135)
Under-compensation	0.550 ***	0.500 ***	0.050
	(0.114)	(0.115)	(0.153)

Note: Reject rates represent the percent of placebo states that rejected the null hypothesis of no effect at the five percent confidence level for β in model 2. The standard errors were clustered at the state level. To correct for a small number of clusters (states), the critical values used for the five percent confidence level were based on a t-distribution with (number of clusters) - (constants per cluster) degrees of freedom rather than on the standard normal distribution (see Angrist and Pischke, 2009, 319-323). Standard errors of the rejection rates are in parentheses. *** indicates significance at the 1% confidence level that the rejection rate is different from 0.05 for weighted and unweighed columns, or that the rejection rates are different from each other for the difference column; ** indicates significance at the 5% confidence level that the rejection rate is different from 0.05 for weighted and unweighed columns, or that the rejection rates are different from each other for the difference column; * indicates significance at the 10% confidence level that the rejection rate is different from 0.05 for weighted and unweighed columns, or that the rejection rates are different from each other for the difference column.

application.[111] The Chow Tests are used to test for differing subgroup effects between claims with and without fraud red flags, and between claims in the bottom half of claimed losses and top half of claimed losses.

non-urban and irrigated, non-urban counties are jointly the same"); Choudhury and Radhakrishnan (2009, 17) (testing for the "differential effect on Statistics course performance due to different mathematical background"); and Ma (2011, 17) (to test if "the growth model is different across rich and poor countries").

[111] Toyodo (1974) shows that the Chow-Test (Chow, 1960), which assumes homoscedasticity, is still valid under heteroscedasticity if the sample size of one of the subgroups being tested is large, which is the case here.

5 Results

5.1 Aggregate Results by Quintile of Expected Claimed Loss

Figure 2 shows the closed claim outcomes for California and synthetic California for claims closed in 1977 and 1987 by quantiles of expected claimed loss. Figure 3 then shows the gaps between California and synthetic California, with the gaps in the 1987 panel representing the estimated treatment effects of *Royal Globe*. Based on Figure 3, there appear to be significant results, especially for smaller claims, across every closed claim outcome. The specials paid, total paid, average days until compensation, and fraud red flags all appear to increase, with the largest increases resulting for smaller claims. At the same time, the probability of under-compensation decreases for the smallest claims, while the probability of a physical examination increases for the largest claims.

To provide a test of the significance of these results, placebo (or "falsification") tests are conducted. The placebo test used here is based on the tests used by Abadie and Gardeazabal (2003) and Abadie et al. (2010).[112] After using a synthetic control method to study the economic effects of terrorism on the Basque Country, Abadie and Gardeazabal (2003) check the ability of the synthetic to produce a valid counterfactual by running a placebo test by conducting the same analysis for Catalonia, a region with significantly less terrorist activity than the Basque Country. Further, to test the significance of their findings on the effects of Proposition 99 on per-

[112]Other similar placebo tests were conducted by DiNardo and Pischke (1997) and Bertrand et al. (2004). After Krueger (1993) found that workers who use computers on the job earn 15 to 20 percent more than other workers, many people concluded that employers pay more for people with computer skills. DiNardo and Pischke (1997) test this hypothesis by comparing the estimated wage differential for those who use pencils, staplers, and calculators to the estimated wage differential for those who use computers. The authors show a similar wage differential, which casts doubt on the idea that the wage differential for computer users is caused by increased returns for that specific skill. In addition, to test the reliability of differences-in-differences inference techniques, Bertrand et al. (2004) conduct a similar type of falsification on a long time series by randomly setting the dates of placebo interventions. See Imbens and Wooldridge (2009, 67-72) for a review of the literature on inference techniques in difference-in-differnences studies.

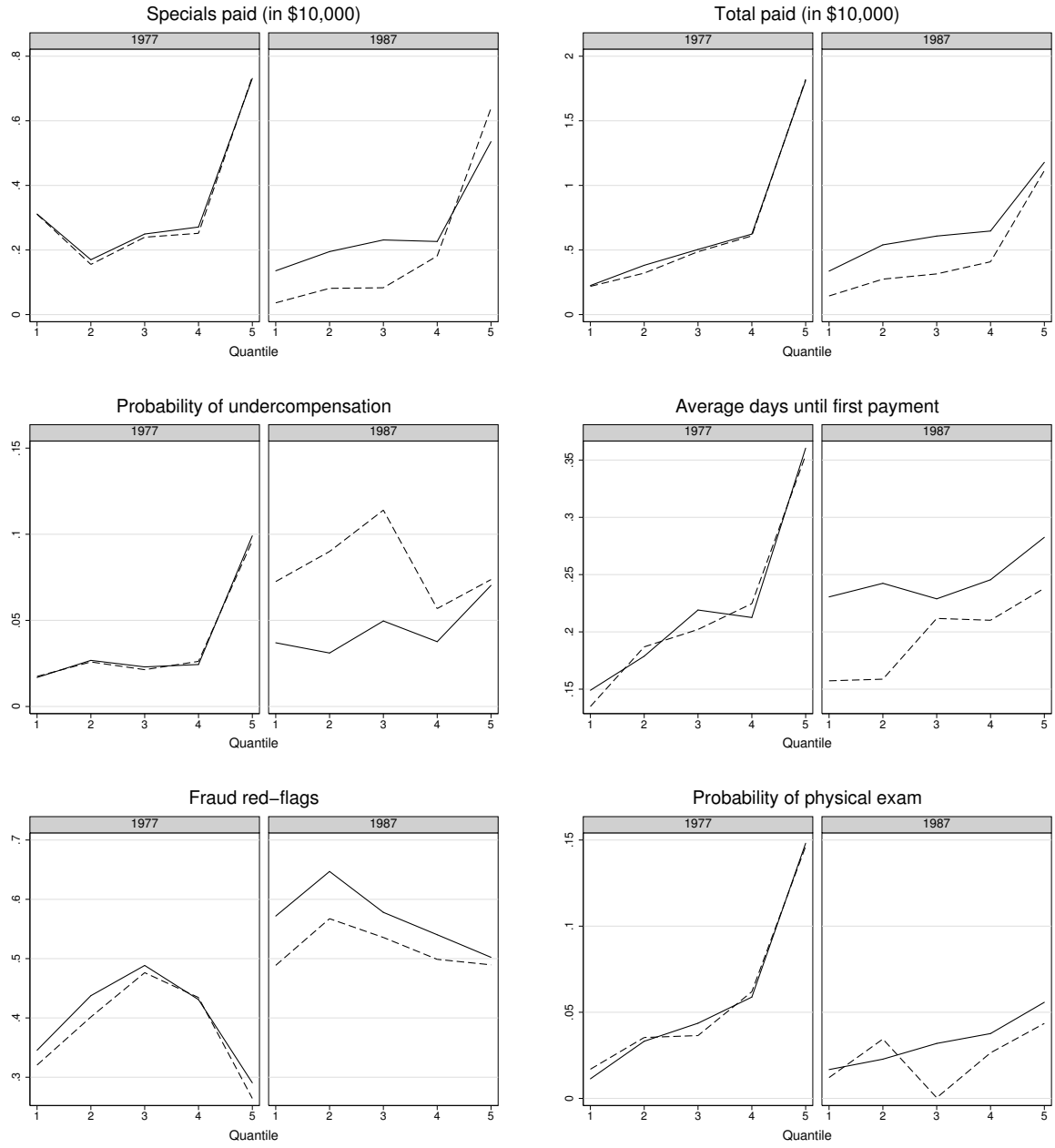

Figure 2: Closed claim outcomes: California vs. synthetic California

Note: California is solid line. Synthetic California is dashed line.

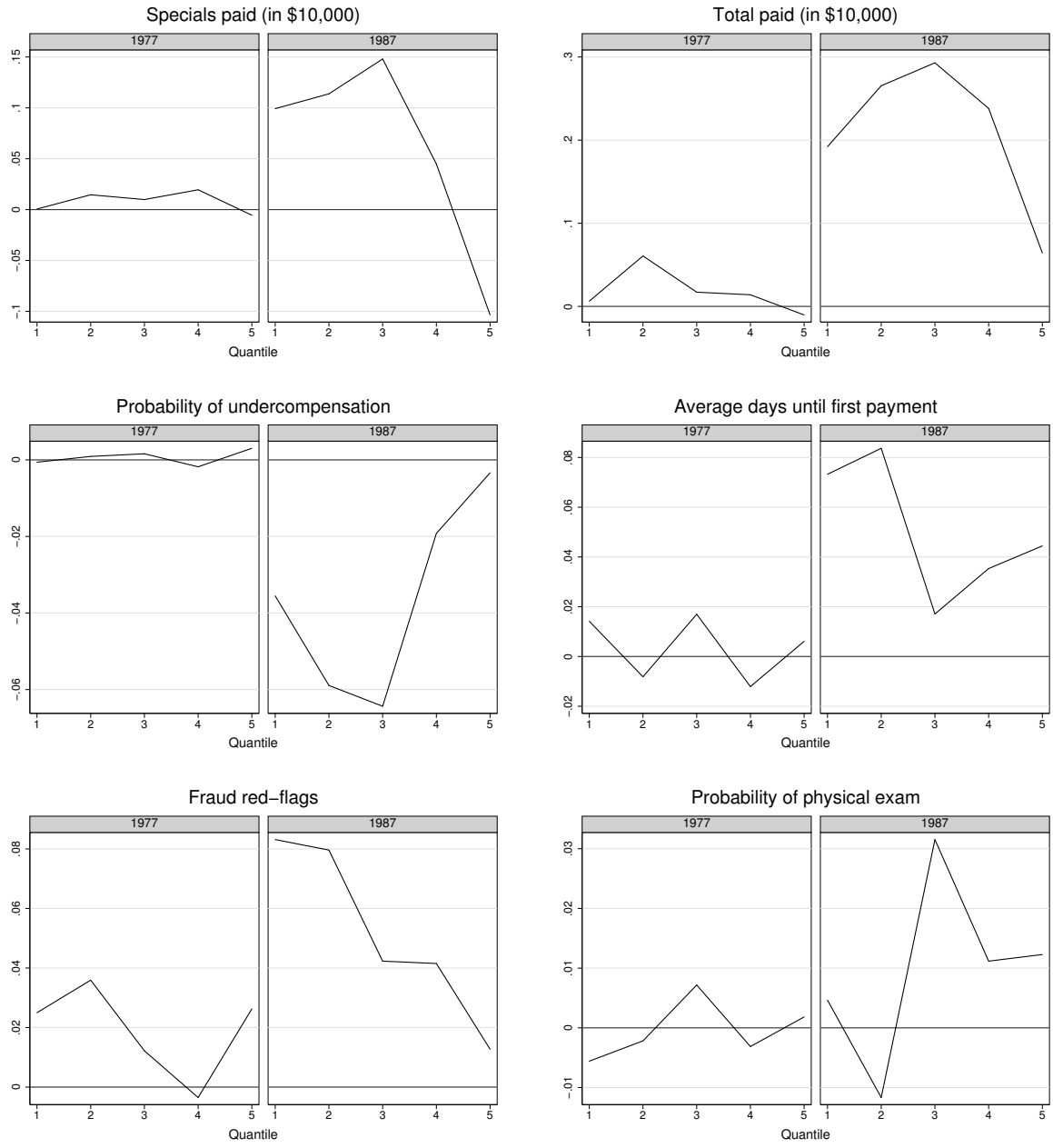

Figure 3: Closed claim outcomes: Gap between California and synthetic California

capita cigarette sale in California, Abadie et al. (2010) apply the synthetic control method to each of 38 potential control states. The authors then compare the gaps in per-capita cigarette sales between each control and its synthetic to the same gap between California and synthetic California.

The goal of the placebo tests is to answer the question posed by Abadie et al. (2010, 501): "How often would we obtain results of this magnitude if we had chosen a state at random for the study instead of [the treated state, California]?" Here, the placebo tests are run by applying the synthetic control method to each potential control state and then comparing the gaps between California and the potential control states. If the post-*Royal Globe* gap for California is "unusually large" relative to the post-*Royal Globe* gap for the control states, then this suggests that there is significant evidence of an effect from California adopting the *Royal Globe* decision (Abadie et al. 2010, 501; also see Abadie and Gardeazabal 2003; Bertrand et al. 2004; Buchmueller et al. 2009).

Figure 4 shows the gaps for California (the solid line) and the gaps for all of the potential control states (the grey lines). Following Abadie et al. (2010), I remove any states that do not have an adequate counterfactual (i.e., states with a pre-*Royal Globe* MSPE of more than ten times that of California).[113] Table 7 shows the post-*Royal Globe* gaps in California by closed claim outcome and by quantile. To make inferences based on these results, I use the distribution of the placebo states to "construct regions of acceptance and rejection for the null hypothesis" that the *Royal Globe* effect is zero (Buchmueller et al., 2009, 19). Any gap estimate that falls below the 2.5^{th} percentile of placebo gaps or above the 97.5^{th} percentile of placebo gaps is deemed significant at the five percent level.

The results show that there are systematic differences in how insurers treat different subgroups of claims, which is consistent with previous models (Bond and

[113] Abadie et al. (2010, 501-503) remove states with a pre period MSPE of 20 times, 5 times, and then 2 times that of California.

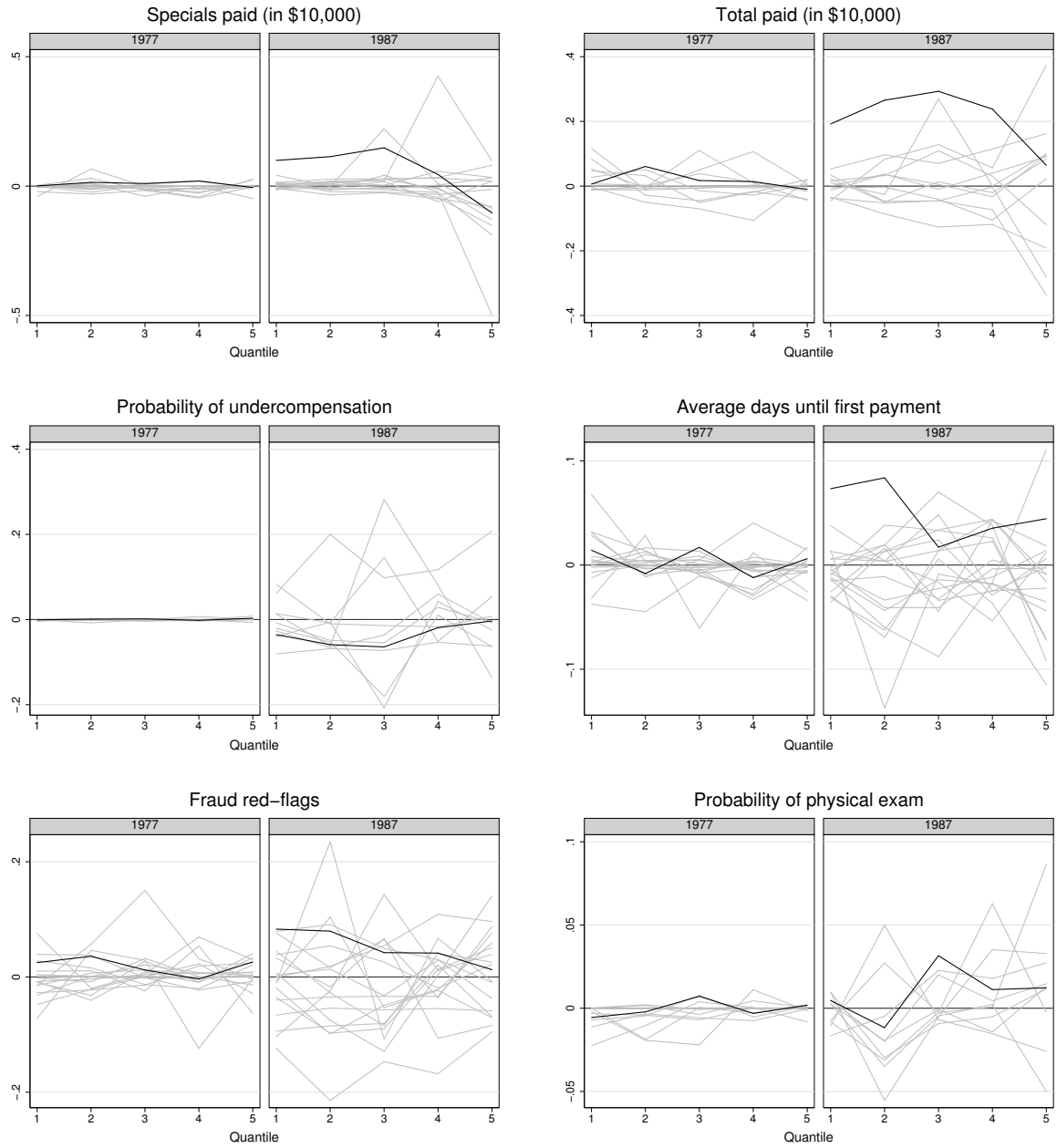

Figure 4: Closed claim outcomes: Gap between California and synthetic California and gaps for all placebo states (discards states with pre-*Royal Globe* MSPE ten times higher than California's)

Note: California gap is black line. Placebo gaps are grey lines.

Table 7: Estimated Closed Claim Outcome Gaps, 1987
(placebo tests, California vs. control states with pre-*Royal Globe* MSPE less than ten times higher than California's)

	Quantile of Expected Claimed Loss				
	1	2	3	4	5
Total paid (in $10,000$)					
	0.192 **	0.265 **	0.293 **	0.238 **	0.064
Placebo tests (other states)					
2.5^{th} percentile	-0.045	-0.086	-0.127	-0.118	-0.337
97.5^{th} percentile	0.053	0.097	0.269	0.115	0.374
Specials paid (in $10,000$)					
	0.099 **	0.114 **	0.148	0.045	-0.104
Placebo tests (other states)					
2.5^{th} percentile	-0.014	-0.034	-0.026	-0.060	-0.497
97.5^{th} percentile	0.042	0.027	0.221	0.425	0.097
Probability of undercompensation					
	-0.036	-0.059	-0.064	-0.019	-0.003
Placebo tests (other states)					
2.5^{th} percentile	-0.081	-0.068	-0.208	-0.053	-0.137
97.5^{th} percentile	0.083	0.200	0.282	0.117	0.208
Average days until first payment (in 1,000)					
	0.073 **	0.084 **	0.017	0.035	0.044
Placebo tests (other states)					
2.5^{th} percentile	-0.036	-0.137	-0.088	-0.054	-0.115
97.5^{th} percentile	0.038	0.038	0.070	0.044	0.111
Fraud red-flags					
	0.083 **	0.080	0.042	0.042	0.013
Placebo tests (other states)					
2.5^{th} percentile	-0.124	-0.214	-0.147	-0.168	-0.095
97.5^{th} percentile	0.079	0.235	0.143	0.109	0.141
Probability of physical exam					
	0.005	-0.012	0.032 **	0.011	0.012
Placebo tests (other states)					
2.5^{th} percentile	-0.017	-0.055	-0.009	-0.015	-0.050
97.5^{th} percentile	0.010	0.050	0.023	0.063	0.086

Note: Estimated using synthetic control method aggregated by state and by quantile of predicted claimed loss as explained in the text. ** indicates significance at the 5% confidence level (i.e., Gap for California above or below the 97.5^{th} or 2.5^{th} percentile of gaps for placebo states).

Crocker, 1997; Crocker and Morgan, 1998; Crocker and Tennyson, 2002). Figure 2 shows that even before *Royal Globe*, insurers were treating different subgroups of claims in different ways. For example, pre-*Royal Globe*, the rate of undercompensation was relatively constant across the first four quantiles of expected claimed loss but spiked for the fifth quantile. Also, the results shown in Figures 3 and 4 show that after *Royal Globe*, insurers changed their settlement practices for different quantiles of expected claimed losses in different (not linear) ways. For example, the average number of days until payment significantly increased for claims with a smaller expected claimed loss but did not significantly change for claims with larger expected claimed losses.

In addition, the results shown in Figure 4, with the associated numerical estimates shown in Table 7, suggest that the rate of insurance company investigations does not decrease (and may even increase for some claims) after *Royal Globe* and that insurers may not be rushing to settle claims as there is a slight increase in the average days until payment for smaller claims. Equally surprising, the rate of fraud red flags in closed claims does not appear to significantly increase either, with a possible exception being for really small claims (i.e., the first quantile of expected claimed loss). These results are contrary to the negative predictions of the effects of increased bad faith liability made by Abraham (1986); Sykes (1996); Tennyson and Warfel (2009).

Further, the results in Figure 4 and Table 7, support the early findings by Hawken et al. (2001); Browne et al. (2004); Tennyson and Asmat (2010) that increased bad faith liability results in an increase in the total amount paid for closed claims. From the first to fourth quantile of expected claimed loss, claims settlements in California increased, on average, by about \$1,920 - \$2,930. In comparison, the 97.5^{th} percentile of placebo gaps in the first quantile was only \$530. Additionally, the *Royal Globe* decision appears to have only resulted in larger payments for smaller claims. The

claims in the fifth quantile of expected claimed loss do not see a significant increase in total payments. This is consistent with Tennyson and Asmat (2010) who find a larger effect of bad faith liability on smaller claims.

5.2 Micro-Level Results

To further test the hypotheses, I apply the synthetic control method to the micro-level data by applying a sample weight to each observation corresponding to the weight determined for that observation's state by the synthetic control method. Using those sample weights, I conduct DD OLS regressions of the form shown in model 2. I first run the regressions on the full sample. I then run the Chow Tests (Chow, 1960) shown in Table 8, which suggest that there are systematic differences in the indemnification strategies insurers use for different subgroups of claims. For every outcome variable the Chow Tests reveal a significant difference at the 1 percent confidence level between the models for fraud and non-fraud red flag claims subgroups. The Chow Tests also reveal significant differences at the 1 percent confidence level for small and large claim subgroups for the days until payment and fraud red flags outcome variables. These results support the above findings from the aggregate results and the previous literature that insurers treat different subgroups of claims in systematically different ways (see Bond and Crocker, 1997; Crocker and Morgan, 1998; Crocker and Tennyson, 2002). The OLS regressions are therefore run on small, large, fraud red flags, and no fraud red flags subgroups. Depending on the subgroups that insurers use to distinguish classes of claims, however, the OLS results may not be as valid as the aggregate results. For reasons discussed below, the aggregate models may do a better job accommodating the different indemnification schedules that insurers have for different subgroups of claims than the OLS models do.

Table 9 shows the results of these regressions. The results support the view

Table 8: Chow Tests for Differing Subgroup Effects
(Small vs. large claims AND Fraud vs. no fraud red flag claims)

	Claims Subgroups	
	Small vs. large	Fraud vs. no fraud
Specials paid	F(34, 5681) = 0.090	F(34, 5681) = 8.963
Total paid	F(34, 7508) = 0.327	F(34, 7508) = 6.076
Probability of undercompensation	F(35, 7202) = 0.807	F(35, 7202) = 12.519
Probability of physical exam	F(38, 8986) = 0.708	F(38, 8986) = 23.448
Average days until first payment	F(33, 5444) = 1.898	F(33, 5444) = 5.779
Fraud red-flags	F(34, 7848) = 177.238	-

Note: All regressions include state fixed effects and controls for individual claim characteristics. Observations are weighed by the weight for their state determined by the synthetic control method. The fraud subgroup contains claims with more than zero fraud red flags. The small subgroup contains claims in the bottom 50th percentile of claimed losses.

that insurers utilize systematically different indemnification schedules for different subgroups of claims. Also, the results are largely consistent with prior theory and OLS results. For example, the specials paid are estimated to increase (Browne et al., 2004; Tennyson and Asmat, 2010), the probability of undercompensation is estimated to decrease (Tennyson and Asmat, 2010), the probability of a physical exam is estimated to decrease (Tennyson and Warfel, 2009), and the number of fraud red flags is estimated to increase (Tennyson and Warfel, 2009). The inferences from the OLS estimates may be more challenging, however, than relying on the usual standard errors.

As with most DD studies where the treatment is at the state level, there exists a problem caused by intraclass correlation (see e.g., White, 1980; Moulton, 1986; Liang and Zeger, 1986; Bell and McCaffrey, 2002; Donald and Lang, 2007; Bertrand et al., 2004; Hansen, 2007a,b; Cameron et al., 2008; Angrist and Lavy, 2002; Angrist and Pischke, 2009; Buchmueller et al., 2009).[114] While this problem can be corrected by clustering standard errors at the state level (which is done here), the asymptotic justification assumes that the number of clusters approaches infinity (Wooldridge,

[114]Bertrand et al. (2004) show that only 36 out of 80 surveyed DD papers with grouped error terms actually address the problem by clustering standard errors or aggregating the data.

Table 9: Difference-in-Difference OLS Estimates

	All	Subgroups			
		Large	Small	No Fraud	Fraud
Specials paid	0.085 ***	0.011	-0.076 ***	0.185 ***	0.001
	(0.029)	(0.033)	(0.020)	(0.051)	(0.082)
R^2	0.895	0.801	0.747	0.879	0.904
N	5,749	3,489	2,260	1,509	4,240
Total paid	0.042	-0.059 ***	-0.010	-0.037	0.101
	(0.065)	(0.022)	(0.124)	(0.056)	(0.118)
R^2	0.755	0.581	0.460	0.754	0.759
N	7,576	4,323	3,253	2,030	5,546
Probability of undercompensation	-0.028 ***	-0.036 ***	-0.015 ***	-0.036 ***	-0.026 ***
	(0.003)	(0.003)	(0.005)	(0.007)	(0.003)
R^2	0.045	0.062	0.034	0.076	0.043
N	7,272	4,161	3,111	2,004	5,268
Probability of physical exam	-0.010 **	-0.007	-0.001	-0.036 ***	0.005 *
	(0.004)	(0.005)	(0.004)	(0.007)	(0.003)
R^2	0.065	0.077	0.021	0.101	0.061
N	9,062	4,941	4,121	2,528	6,534
Average days until first payment	26.602 ***	25.320 *	27.620 ***	-12.265 *	42.155 ***
	(6.221)	(13.020)	(2.808)	(6.722)	(6.995)
R^2	0.176	0.089	0.095	0.173	0.193
N	5,510	3,405	2,105	1,410	4,100
Fraud red-flags	0.030 ***	0.022 *	0.034 ***	-	-
	(0.007)	(0.013)	(0.011)		
R^2	0.496	0.488	0.516	-	-
N	7,916	4,600	3,316	-	-

Note: All regressions include state fixed effects and controls for individual claim characteristics. Observations are weighed by the weight for their state determined by the synthetic control method. Standard errors are reported below the coefficient estimates. Standard errors clustered at the state level. *** indicates statistical significance at the 1 percent confidence level; ** indicates significance at the 5 percent confidence level; and * indicates significance at the 10 percent confidence level.

2003; Cameron et al., 2008; Angrist and Pischke, 2009; Buchmueller et al., 2009). When this is not the case, the standard errors are downward biased and there is a tendency to over reject the null hypothesis.[115] Clearly, the assumption that the number of clusters trends to infinity is not the case in this situation. Note that the rejection rates in the placebo test in Table 6, above, are significantly higher than 5 percent.

Therefore, to make inferences on the significance of the DD OLS results, I continue to implement placebo tests like the ones instituted above. Consistent with Buchmueller et al. (2009), who faced a similar clustering problem, I construct acceptance and rejection regions based on the distributions of placebo estimates from the OLS regressions. If the effect on California is significantly greater than the estimated effects for the placebo states (outside of the 2.5^{th} - 97.5^{th} percentile range of placebo estimates), then the impact is deemed significant. This method also resembles the placebo tests conducted by Bertrand et al. (2004) and Abadie et al. (2010) and does not rely on asymptotic assumptions. As Abadie et al. (2010, 497) argue, this type of "inferential exercise is exact in the sense that, regardless of the number of available comparison regions, time periods, and whether the data are individual or aggregate, it is always possible to calculate the exact distribution of the estimated effect of the placebo interventions" (see also, Buchmueller et al., 2009, 18). This approach produces more "conservative inferences" (Buchmueller et al., 2009, 19).

Table 10 shows the DD OLS estimates from Table 9 and compares the magnitude of the estimated coefficient from California to the distribution of the magnitudes from the same models for each potential control state. Doing so reduces the amount of statistically significant estimates to zero. The estimates for some

[115]See Angrist and Pischke (2009, 319-323) for a discussion of potential corrections to the clustering problem.

states, however, may rest on a relatively small number of claims.[116] While this, in and of itself, may be a finding, the more likely explanation is that the subgroups used (i.e., small vs. large claims, and fraud vs. no fraud red flags claims) do not accurately represent the subgroups that insurers use when creating their different indemnification schedules. If insurers do use systematically different indemnification schedules (which the above results and prior literature suggests they do), then the OLS results will not provide valid estimates, unless the correct subgroups are identified and estimated separately (or with fully interacted models).

I argue that in this situation the most valid estimates are from the aggregate results. Both Bond and Crocker (1997) and Crocker and Morgan (1998) develop models of insurance indemnification that treat claims in systematically different ways based on the claimed loss. The aggregate analysis groups similar claims into quantiles of expected claimed loss and allows for completely different effects in each quantile. If the quantile groupings are roughly similar to the subgroups that insurers use, which they may be if insurers follow Bond and Crocker (1997) and Crocker and Morgan (1998), then the estimated results will approximate the accurate treatment effects. Further, Figure 5 confirms that fraud red flags tend to cluster in different quantiles. Therefore, if insurers use fraud red flags, alternatively or in conjunction with claimed amount, to divide claims into subgroups, then the aggregate results by quantile may still be the more accurate measure of treatment effects. Replicating those cutoffs with the DD OLS models would involve conducting separate analyses for each of the created subgroups and would thus lack subsample variation in the variables shown in Table 2. For these reasons, the aggregate results may be the more accurate estimates.

[116]The results are robust to only using statistically significant estimates (based on normal standard errors) when constructing the distributions of placebo estimates.

Table 10: Difference-in-Differences OLS Estimates with Placebo Tests
(California vs. control states with 1977 MSPE less than ten times higher than California's)

	All	Subgroups			
		Large	Small	Fraud	No Fraud
Specials paid					
	0.085	0.011	-0.076	0.185	0.001
Placebo tests (other states)					
2.5^{th} percentile	-0.159	-0.298	-0.618	-0.301	-0.264
97.5^{th} percentile	0.128	0.226	0.661	0.195	0.480
Total paid					
	0.042	-0.059	-0.010	-0.037	0.101
Placebo tests (other states)					
2.5^{th} percentile	-0.293	-0.195	-0.912	-0.756	-0.493
97.5^{th} percentile	0.389	0.247	0.604	0.361	0.789
Probability of undercompensation					
	-0.028	-0.036	-0.015	-0.036	-0.026
Placebo tests (other states)					
2.5^{th} percentile	-0.051	-0.295	-0.049	-0.112	-0.076
97.5^{th} percentile	0.179	0.253	0.071	0.244	0.124
Probability of physical exam					
	-0.010	-0.007	-0.001	-0.036	0.005
Placebo tests (other states)					
2.5^{th} percentile	-0.108	-0.182	-0.184	-0.192	-0.315
97.5^{th} percentile	0.026	0.038	0.060	0.091	0.028
Average days until first payment					
	26.602	25.320	27.620	-12.265	42.155
Placebo tests (other states)					
2.5^{th} percentile	-36.523	-82.732	-92.897	-112.053	-90.996
97.5^{th} percentile	138.802	97.116	158.120	142.627	129.401
Fraud red-flags					
	0.030	0.022	0.034	-	-
Placebo tests (other states)					
2.5^{th} percentile	-0.085	-0.072	-0.104	-	-
97.5^{th} percentile	0.100	0.138	0.141	-	-

Note: All regressions include state fixed effects and controls for individual claim characteristics. Observations are weighed by the weight for their state determined by the synthetic control method. ** indicates significance at the 5% confidence level (i.e., Gap for California above or below the 97.5th or 2.5th percentile of gaps for placebo states).

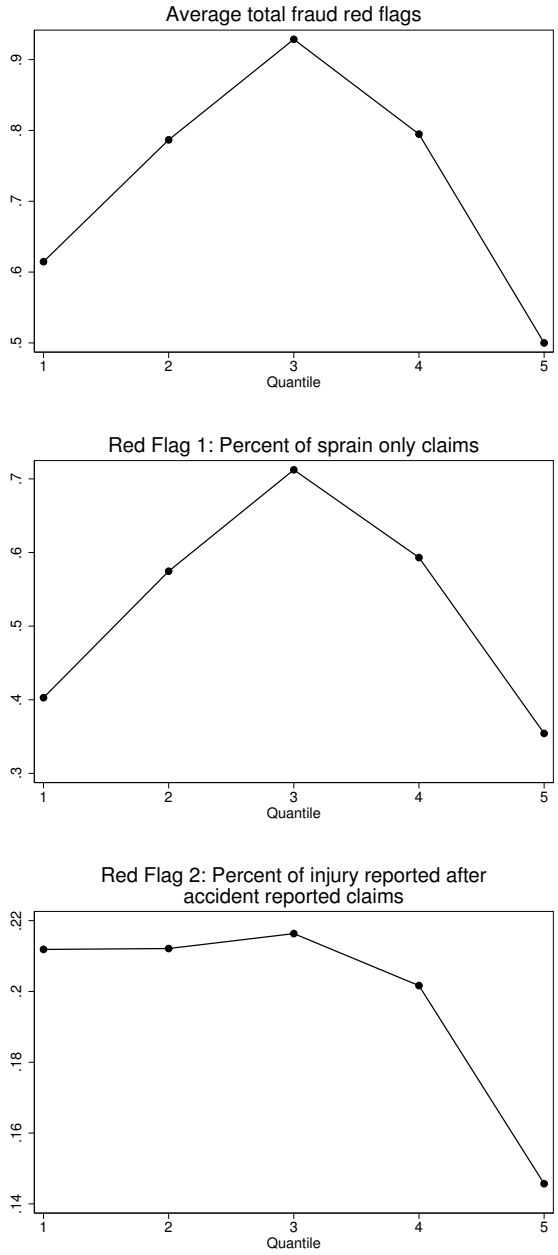

Figure 5: Distribution of fraud red flags across quantiles of expected claimed loss

6 Conclusion

This paper has analyzed the effects of adopting the *Royal Globe* doctrine (increasing insurance bad faith liability) on automobile bodily injury insurance claims outcomes. The study provides new evidence over a wide range of claim outcomes to paint a fuller picture of the effects of increased bad faith liability. The use of a "natural experiment" created by the "random" nature of the *Royal Globe* decision and the utilization of the powerful synthetic control method allows the study to make causal estimates on observed claims settlement practices, an advantage over previous work.

The empirical results support two important findings. First, consistent with previous theories of claims settlement practices (Bond and Crocker, 1997; Crocker and Morgan, 1998; Crocker and Tennyson, 2002), insurers appear to treat different subgroups of claims in systematically different ways. The aggregate analysis by quantiles of expected claimed loss and the micro-level Chow-Tests for differing subgroup effects for fraud vs. non-fraud red flag claims and small vs. large claims both support this conclusion.

Second, the empirical results are largely consistent with an efficient application of bad faith liability (i.e., the application of potential punitive damages). The aggregate analysis by quantiles of expected claimed loss and the micro-level OLS models suggest that the predicted negative effects of increased bad faith liability on insurers' claim settlement practices do not appear to be severe (or significantly exist) (see Abraham, 1986; Sykes, 1996; Tennyson and Warfel, 2009). I generally find no significant increase in the prevalence of fraud red flags and no significant decrease (and possibly an increase) in the rate of insurance company investigations for closed claims. Similarly, there is no evidence that insurers are rushing to settle claims as there is no decrease in the average days until payment and even a slight increase for the smallest claims. Also, the aggregate analysis finds that the

total amount paid significantly increases for claims in the lowest four quintiles of expected claimed loss. These claims are still small as the 99^{th} percentile of claimed losses in the fourth quintile is only $6,207. This is consistent with the efficient use of punitive damages to protect small claims that are systematically underpaid, where the "aggregate damages may be significant, [but] no single victim of the wrongful act [without potential punitive damages] has sufficient damages to make suing worthwhile" (Posner, 2006, 746).

In the future, as people continue to debate the issue of consumer protection in the insurance industry, these results suggest that a more market based approach may be appropriate.[117] Individual consumers are the first person (and likely only person) to know if they are underpaid and can thus *privately* bring any necessary suits against insurers.[118] Standard breach of contact suits should work fine for larger claims and the additional expected damages from potential punitive damages should give small claims that are underpaid enough of an incentive to file suit. As long as the punitive damages are correctly applied to the "rare" subgroup of claims where additional damages are necessary to deter opportunistic behavior by the insurer (Posner, 2006, 746), a reliance on the private market and private enforcement should be efficient. These results suggest that insurers are not *too* deterred that they start paying more fraudulent claims and doing less investigations, and that the true beneficiaries of bad faith liability may be those small claims that absent the additional punitive damages would have no incentive to sue.

Despite these interesting findings, and even with the advantages of detailed micro-level data and a "quasi-experimental" treatment, more research is still needed.

[117]For a discussion of the benefits of ex post liability versus ex ante regulations see Calabresi (1970); Weitzman (1974); Wittman (1977); Shavell (1984); Rose-Ackerman (1991); Söllner (1994, 1996); Schwarze (1996).

[118]Private enforcement is optimal in situations where private parties know the identities of their injurers, their harm, and are the lowest cost enforcers (Shavell, 1984, 1993). In the insurance context, private parties posses all of the necessary information and have the incentive to pursue enforcement at any point where the marginal benefit of enforcement for them is greater than the marginal cost of enforcement to them (see Becker and Stigler, 1974).

While I show findings consistent with efficiency, the lack of data on denied claims and settlement negotiations precludes ruling out alternative scenarios. For example, even though the rate of insurers' investigations for closed claims does not decrease, it is possible that insurers just deny more claims and maintain their investigation rate among the smaller subgroup of claims that they pay some amount to. Therefore, to assess the full net social costs and benefits of increased insurance bad faith liability, more research, especially on claimant practices and the effects on all claims, would be useful.

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Appendix Table A.1: State Liability Laws: 1971-1987

State	Bad Faith Liability			Terms of Liability[a]		
	Statute[b]	Third Party[c]	*Royal Globe*	Tort	No Fault	Add-On
Alabama	No	1921	No	Yes	No	No
Alaska	No Ruling	1980	No	Yes	No	No
Arizona	1982	1957	No	Yes	No	No
Arkansas	No Ruling	1954	No	No	No	Yes
California	1979	1958	1979-1988	Yes	No	No
Colorado	Rejected (1982)	1984	No	-1974	1974-	No
Connecticut	1982	1966	No	-1973	1973-	No
Delaware	No Ruling	1972	No	No	No	Yes
District of Columbia	No	No Ruling	No	-1983	1983-1986	1986-
Florida	1982	1980	No	-1972	1972-	No
Georgia	No	1984	No	-1975	1975-	No
Hawaii	No Ruling	No Ruling	No	-1974	1974-	No
Idaho	No Ruling	1971	No	Yes	No	No
Illinois	Rejected (1984)	1979	No	Yes	No	No
Indiana	No Ruling	1965	No	Yes	No	No
Iowa	Rejected (1982)	1982	No	Yes	No	No
Kansas	Rejected (1980)	1974	No	-1974	1974-	No
Kentucky	No	1968	No	-1975	1975-	No
Louisiana	No	1974	No	Yes	No	No
Maine	No	1950	No	Yes	No	No
Maryland	No	1967	No	No	No	Yes
Massachusetts	1984	1959	No	-1971	1971-	No
Michigan	Rejected (1986)	1984	No	-1973	1973-	No
Minnesota	Rejected (1986)	1983	No	-1975	1975-	No
Mississippi	No	Rejected (1939)	No	Yes	No	No
Missouri	Rejected (1981)	1950	No	Yes	No	No

Appendix Table A.1: State Liability Laws: 1971-1987, cont.

	Bad Faith Liability			Terms of Liability[a]		
State	Statute[b]	Third Party[c]	*Royal Globe*[d]	Tort	No Fault	Add-On
Montana	1983	1984	1983	Yes	No	No
Nebraska	No Ruling	1976	No	Yes	No	No
Nevada	Rejected (1985)	No Ruling	No	Yes	No	No
New Hampshire	Rejected (1978)	1971	No	Yes	No	No
New Jersey	Rejected (1982)	1976	No	-1973	1973-	No
New Mexico	Rejected (1984)	1984	No	Yes	No	No
New York	Rejected (1982)	1972	No	-1974	1974-	No
North Carolina	No Ruling	1979	No	Yes	No	No
North Dakota	1985	No Ruling	No	-1976	1976-	No
Ohio	No	1962	No	Yes	No	No
Oklahoma	No	1957	No	Yes	No	No
Oregon	Rejected (1984)	1985	No	No	No	Yes
Pennsylvania	Rejected (1981)	1957	No	No	-1984	1984-
Rhode Island	1981	1983	No	Yes	No	No
South Carolina	Rejected (1984)	1961	No	No	No	Yes
South Dakota	No	1973	No	No	No	Yes
Tennessee	No Ruling	1968	No	Yes	No	No
Texas	Unclear	1929	No	No	No	Yes
Utah	No	1967	No	-1974	1974-	No
Vermont	Rejected (1981)	1986	No	Yes	No	No
Virginia	1983	1966	No	No	No	Yes
Washington	Rejected (1986)	1974	No			
West Virginia	1981	No Ruling	1981	Yes	No	No
Wisconsin	No Ruling	1973	No	Yes	No	No
Wyoming	No	1964	No	Yes	No	No

[a]Source: All-Industry Research Advisory Committee (1979) and All-Industry Research Advisory Council (1989)

[b]Source: Ashley (1987, 89-134)

[c]Source: Ashley (1987, 89-134)

[d]Source: Stempel (2006, 9.05-9.06)

Table A.2: Claims Characteristics by Quantile by Year

Variable	q	1977	1987	Difference
Ln(Claimed loss)	1	5.256	5.763	-0.507 ***
		(0.041)	(0.023)	(0.051)
	2	5.856	6.396	-0.540 ***
		(0.043)	(0.036)	(0.057)
	3	6.263	6.658	-0.396 ***
		(0.043)	(0.045)	(0.062)
	4	6.597	6.818	-0.221 ***
		(0.042)	(0.031)	(0.051)
	5	7.883	7.789	0.094 *
		(0.044)	(0.025)	(0.053)
Acc. Location: Central City	1	0.304	0.323	-0.020
		(0.016)	(0.008)	(0.018)
	2	0.316	0.430	-0.114 ***
		(0.016)	(0.013)	(0.021)
	3	0.410	0.402	0.007
		(0.017)	(0.017)	(0.024)
	4	0.443	0.314	0.129 ***
		(0.017)	(0.013)	(0.021)
	5	0.332	0.399	-0.067 ***
		(0.016)	(0.009)	(0.019)
Acc. Location: Medium City	1	0.248	0.297	-0.049 ***
		(0.015)	(0.008)	(0.017)
	2	0.247	0.233	0.014
		(0.015)	(0.011)	(0.019)
	3	0.203	0.239	-0.036 *
		(0.014)	(0.015)	(0.020)
	4	0.189	0.313	-0.125 ***
		(0.013)	(0.013)	(0.019)
	5	0.206	0.236	-0.030 *
		(0.014)	(0.008)	(0.016)
Acc. Location: Small Town	1	0.182	0.106	0.075 ***
		(0.013)	(0.005)	(0.012)
	2	0.171	0.070	0.101 ***
		(0.013)	(0.007)	(0.013)
	3	0.128	0.090	0.038 **
		(0.011)	(0.010)	(0.015)
	4	0.129	0.094	0.034 **
		(0.012)	(0.008)	(0.014)
	5	0.161	0.080	0.081 ***
		(0.013)	(0.005)	(0.011)

Table A.2: Claims Characteristics by Quantile by Year, cont.

Variable	q	1977	1987	Difference
Acc. Location: Rural	1	0.118	0.067	0.051 ***
		(0.011)	(0.004)	(0.010)
	2	0.079	0.050	0.029 ***
		(0.009)	(0.006)	(0.010)
	3	0.083	0.068	0.015
		(0.009)	(0.009)	(0.013)
	4	0.077	0.077	0.000
		(0.009)	(0.007)	(0.012)
	5	0.166	0.077	0.088 ***
		(0.013)	(0.005)	(0.011)
Claimant is Driver	1	0.598	0.467	0.132 ***
		(0.017)	(0.008)	(0.019)
	2	0.664	0.654	0.010
		(0.016)	(0.013)	(0.021)
	3	0.795	0.640	0.156 ***
		(0.014)	(0.017)	(0.022)
	4	0.811	0.618	0.194 ***
		(0.013)	(0.013)	(0.020)
	5	0.640	0.744	-0.104 ***
		(0.016)	(0.008)	(0.017)
Claimant is Passenger	1	0.356	0.482	-0.125 ***
		(0.016)	(0.008)	(0.019)
	2	0.311	0.306	0.005
		(0.016)	(0.012)	(0.020)
	3	0.180	0.301	-0.122 ***
		(0.013)	(0.016)	(0.021)
	4	0.162	0.324	-0.163 ***
		(0.013)	(0.013)	(0.019)
	5	0.276	0.195	0.081 ***
		(0.015)	(0.007)	(0.016)
Claimant is Other	1	0.005	0.006	-0.001
		(0.002)	(0.001)	(0.003)
	2	0.001	0.015	-0.014 ***
		(0.001)	(0.003)	(0.004)
	3	0.003	0.010	-0.006
		(0.002)	(0.003)	(0.004)
	4	0.005	0.010	-0.005
		(0.002)	(0.003)	(0.004)
	5	0.008	0.013	-0.005
		(0.003)	(0.002)	(0.004)

Table A.2: Claims Characteristics by Quantile by Year, cont.

Variable	q	1977	1987	Difference
Number of Other Vehicles in Accident	1	1.111	1.080	0.031
		(0.019)	(0.009)	(0.021)
	2	1.199	1.286	-0.087
		(0.021)	(0.042)	(0.056)
	3	1.213	1.213	0.000
		(0.019)	(0.056)	(0.057)
	4	1.246	1.175	0.071
		(0.023)	(0.036)	(0.049)
	5	1.101	1.205	-0.104 *
		(0.023)	(0.028)	(0.053)
Male Claimant	1	0.346	0.362	-0.016
		(0.016)	(0.008)	(0.018)
	2	0.341	0.500	-0.159 ***
		(0.016)	(0.013)	(0.021)
	3	0.567	0.585	-0.018
		(0.017)	(0.017)	(0.024)
	4	0.680	0.401	0.280 ***
		(0.016)	(0.013)	(0.021)
	5	0.647	0.557	0.090 ***
		(0.016)	(0.009)	(0.019)
Claimant's Age	1	26.687	23.817	2.870 ***
		(0.420)	(0.223)	(0.498)
	2	28.256	36.514	-8.258 ***
		(0.404)	(0.437)	(0.639)
	3	31.083	38.237	-7.155 ***
		(0.393)	(0.631)	(0.733)
	4	38.071	34.287	3.784 ***
		(0.503)	(0.450)	(0.695)
	5	36.833	36.000	0.834
		(0.580)	(0.264)	(0.585)
Extent of Disability: Temporary	1	0.229	0.042	0.187 ***
		(0.014)	(0.003)	(0.010)
	2	0.852	0.362	0.490 ***
		(0.012)	(0.013)	(0.019)
	3	0.955	0.615	0.340 ***
		(0.007)	(0.017)	(0.018)
	4	0.882	0.600	0.282 ***
		(0.011)	(0.013)	(0.019)
	5	0.559	0.753	-0.193 ***
		(0.017)	(0.008)	(0.017)

Table A.2: Claims Characteristics by Quantile by Year, cont.

Variable	q	1977	1987	Difference
Extent of Disability: Permanent Partial	1	0.000	0.000	0.000
		(0.000)	(0.000)	(0.000)
	2	0.007	0.002	0.005 *
		(0.003)	(0.001)	(0.003)
	3	0.012	0.005	0.007
		(0.004)	(0.002)	(0.004)
	4	0.067	0.031	0.036 ***
		(0.009)	(0.005)	(0.009)
	5	0.304	0.125	0.179 ***
		(0.016)	(0.006)	(0.014)
Extent of Disability: Permanent Total	1	0.000	0.000	0.000
		(0.000)	(0.000)	(0.000)
	2	0.000	0.000	0.000
		(0.000)	(0.000)	(0.000)
	3	0.000	0.000	0.000
		(0.000)	(0.000)	(0.000)
	4	0.001	0.000	0.001
		(0.001)	(0.000)	(0.001)
	5	0.015	0.007	0.009 **
		(0.004)	(0.001)	(0.004)
Extent of Disability: Fatal	1	0.000	0.000	0.000
		(0.000)	(0.000)	(0.000)
	2	0.000	0.000	0.000
		(0.000)	(0.000)	(0.000)
	3	0.000	0.000	0.000
		(0.000)	(0.000)	(0.000)
	4	0.000	0.001	-0.001
		(0.000)	(0.001)	(0.001)
	5	0.097	0.016	0.081 ***
		(0.010)	(0.002)	(0.007)
Injury: Sprain	1	0.482	0.650	-0.168 ***
		(0.017)	(0.008)	(0.018)
	2	0.702	0.883	-0.181 ***
		(0.016)	(0.009)	(0.016)
	3	0.893	0.819	0.074 ***
		(0.010)	(0.014)	(0.017)
	4	0.787	0.784	0.002
		(0.014)	(0.011)	(0.018)
	5	0.599	0.829	-0.230 ***
		(0.017)	(0.007)	(0.016)

Table A.2: Claims Characteristics by Quantile by Year, cont.

Variable	q	1977	1987	Difference
Injury: Fracture	1	0.008	0.004	0.004
		(0.003)	(0.001)	(0.003)
	2	0.010	0.017	-0.006
		(0.003)	(0.003)	(0.005)
	3	0.033	0.043	-0.010
		(0.006)	(0.007)	(0.009)
	4	0.114	0.063	0.051 ***
		(0.011)	(0.007)	(0.012)
	5	0.380	0.144	0.236 ***
		(0.017)	(0.006)	(0.015)
Injury: Laceration	1	0.113	0.306	-0.193 ***
		(0.011)	(0.008)	(0.017)
	2	0.092	0.169	-0.077 ***
		(0.010)	(0.010)	(0.015)
	3	0.059	0.220	-0.161 ***
		(0.008)	(0.015)	(0.016)
	4	0.086	0.235	-0.149 ***
		(0.010)	(0.012)	(0.016)
	5	0.145	0.211	-0.066 ***
		(0.012)	(0.007)	(0.015)
Injury: Other	1	0.496	0.143	0.353 ***
		(0.017)	(0.006)	(0.015)
	2	0.352	0.136	0.216 ***
		(0.016)	(0.009)	(0.017)
	3	0.220	0.171	0.049 **
		(0.014)	(0.013)	(0.019)
	4	0.271	0.183	0.088 ***
		(0.015)	(0.011)	(0.018)
	5	0.339	0.237	0.102 ***
		(0.016)	(0.008)	(0.017)
Inpatient Hospitalization: 1 Night	1	0.006	0.001	0.005 ***
		(0.003)	(0.000)	(0.002)
	2	0.005	0.004	0.000
		(0.002)	(0.002)	(0.003)
	3	0.006	0.012	-0.007
		(0.003)	(0.004)	(0.005)
	4	0.022	0.018	0.004
		(0.005)	(0.004)	(0.006)
	5	0.030	0.027	0.004
		(0.006)	(0.003)	(0.006)

Table A.2: Claims Characteristics by Quantile by Year, cont.

Variable	q	1977	1987	Difference
Inpatient Hospitalization: 2-7 Days	1	0.001	0.002	-0.001
		(0.001)	(0.001)	(0.002)
	2	0.006	0.009	-0.003
		(0.003)	(0.002)	(0.004)
	3	0.010	0.016	-0.006
		(0.003)	(0.004)	(0.006)
	4	0.075	0.035	0.041 ***
		(0.009)	(0.005)	(0.010)
	5	0.246	0.089	0.157 ***
		(0.015)	(0.005)	(0.012)
Inpatient Hospitalization: Over 7 Days	1	0.000	0.000	0.000
		(0.000)	(0.000)	(0.000)
	2	0.001	0.000	0.001
		(0.001)	(0.000)	(0.001)
	3	0.000	0.000	0.000
		(0.000)	(0.000)	(0.000)
	4	0.006	0.001	0.005 **
		(0.003)	(0.001)	(0.002)
	5	0.333	0.070	0.264 ***
		(0.016)	(0.005)	(0.012)
Wage Loss Claimed	1	0.654	0.013	0.641 ***
		(0.016)	(0.002)	(0.009)
	2	0.854	0.028	0.826 ***
		(0.012)	(0.004)	(0.011)
	3	0.918	0.122	0.797 ***
		(0.009)	(0.011)	(0.015)
	4	0.932	0.573	0.359 ***
		(0.009)	(0.013)	(0.018)
	5	0.796	0.879	-0.083 ***
		(0.014)	(0.006)	(0.013)
Medical Loss Claimed	1	0.293	0.978	-0.685 ***
		(0.016)	(0.002)	(0.009)
	2	0.133	0.979	-0.846 ***
		(0.012)	(0.004)	(0.010)
	3	0.084	0.968	-0.884 ***
		(0.009)	(0.006)	(0.011)
	4	0.154	0.986	-0.832 ***
		(0.012)	(0.003)	(0.011)
	5	0.353	0.992	-0.639 ***
		(0.016)	(0.002)	(0.009)

Table A.2: Claims Characteristics by Quantile by Year, cont.

Variable	q	1977	1987	Difference
Other Expenses Claimed	1	0.052	0.017	0.035 ***
		(0.008)	(0.002)	(0.006)
	2	0.019	0.051	-0.033 ***
		(0.005)	(0.006)	(0.008)
	3	0.013	0.103	-0.091 ***
		(0.004)	(0.011)	(0.011)
	4	0.035	0.085	-0.050 ***
		(0.006)	(0.008)	(0.011)
	5	0.166	0.132	0.034 **
		(0.013)	(0.006)	(0.013)

Note: Standard errors are in parentheses. Differences: *** indicates statistical significance at the 1 percent confidence level; ** indicates significance at the 5 percent confidence level; and * indicates significance at the 10 percent confidence level.